This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

ON

THE NATURE OF CHOLERA,

AS

A GUIDE TO TREATMENT.

ON

THE NATURE OF CHOLERA,

AS

A GUIDE TO TREATMENT.

BY

WILLIAM SEDGWICK, M.R.C.S. AND L.S.A.
SURGEON TO THE ST. MARYLEBONE PROVIDENT DISPENSARY;
FELLOW OF THE ROYAL MEDICO-CHIRURGICAL, PATHOLOGICAL AND
HARVEIAN SOCIETIES.

Second Issue.

WITH A NEW SECTION ON TREATMENT.

BIBLIOTHECA · BODLEIANA

LONDON:
WALTON AND MABERLY,
UPPER GOWER STREET, AND IVY LANE, PATERNOSTER ROW.

1866.

157. o. 91.

LONDON:
WERTHEIMER AND CO., PRINTERS,
CIRCUS PLACE, FINSBURY CIRCUS.

PREFACE.

The following remarks on the nature of cholera are intended to illustrate the theory, which refers the disease to functional disorder of the central parts of the sympathetic nervous system, excited through the medium of the stomach.

The origin of this theory may be traced to a very early period in the history of cholera. It was distinctly alluded to by some of the medical officers in the East India Company's service, during the first outbreak of the disease in India; but it was not until after the appearance of the disease in Europe, that the attention of medical men can be said to have been specially directed to establish its correctness. The success which has hitherto attended their efforts has been incomplete, and there are still many difficulties connected with the subject, which require to be removed, before it can be expected that the theory proposed will be generally approved of.

The most conclusive arguments in its favour, will be found to hinge upon a correct interpretation of some of the more remarkable symptoms of the disease, as the suppression of urine, the altered condition of the blood, the state of the circulation, and the flux from the stomach and bowels ; considerable attention has therefore been bestowed on this branch of the subject; and in the attempt which has been made to elucidate these phenomena, it may be thought that others equally essential to the disease have not received their due amount of consideration : if this should be the case, it is not because such symptoms are regarded as less important, but only from their being less distinctive of cholera.

The writer feels that in an inquiry which has baffled many of the most distinguished members of the profession, he has no right to anticipate any great success ; but his object will be attained, if he can contribute to explain some of the phenomena of the disease which are admitted to be obscure; and, by so doing, to prepare the way for a more rational, and it may be, a more successful mode of treatment.

The new section on treatment has been introduced for the purpose of showing more fully than on the former occasion, that it might be useful to revive the neglected practice of bleeding in cholera. For whilst, on the one side, there is an admitted failure of the various plans of treatment which have been suggested and tried; there is, on the other side, undoubted evidence to prove that a very large number of patients afflicted with cholera have recovered after they were bled: even although the bleeding was tried late in that stage of collapse, when all drugs administered internally are very generally admitted to be equally inert.

12, Park Place, Upper Baker Street,
August, 1866.

CONTENTS.

PAGE

INTRODUCTION 1

ON THE PROBABLE SEAT OF THE PRIMARY AFFECTION IN CHOLERA 7

I.—Cholera as the result of structural disease of the semilunar ganglia, solar plexus, and adjoining portions of the sympathetic nervous system 10

II.—Cholera as the result of functional disorder of the sympathetic nervous system, produced through the medium of the lungs 13

On the condition of the lungs in cholera 15

III.—Cholera as the result of functional disorder of the sympathetic nervous system, produced through the medium of the alimentary canal 23

I.—The general relation between cholera and other disorders which admit of being directly referred to an affection of the sympathetic nervous system, produced through the medium of the stomach 25

II.—The analysis of particular phenomena in cholera . . . 40

1. The suppression of urine 41

(*a*) The probable cause of the appearance usually presented by the urinary organs, after death occurring in the collapsed stage of the disease . 45

(*b*) The extent to which the formation of urea in the blood is affected, after its elimination by the kidneys is arrested 57

PAGE
Analysis of particular phenomena (*continued*).
2. The altered condition of the blood 77
(*a*) On the chemical reaction of the blood in cholera . 82
(*b*) On the colour of the blood in cholera . . . 84
(*c*) On the coagulation of the blood in cholera . . 90
3. The flux from the stomach and bowels 106
(*a*) The characters of the fluid discharged from the alimentary canal 108
(*b*) The probable cause of the flux 115
(*c*) Its relation to collapse 135
4. On the failure of the circulation in cholera . . . 142
On Organic Function in relation to Cholera 169
Treatment 172
Conclusion 199

ON THE NATURE OF CHOLERA.

INTRODUCTION.

There is probably no disease respecting which more conflicting opinions have been entertained than cholera. Its origin has been ascribed to almost all the causes that have been at any time supposed capable of giving rise to disease, while the viscera of the head, the chest, and the abdomen have been respectively referred to as its primary seat. As it would be contrary to the object of the present treatise to enter into a description of the merits of these different theories, I shall proceed to notice those only which have represented cholera to be due to an affection of the sympathetic nervous system.

The very indefinite information we possess respecting the functions of the sympathetic nerve in health, must necessarily increase the difficulty of investigating its diseases, and the strongest arguments adduced on some points connected with the subject, will consequently be found to amount to scarcely more than good presumptive evidence in favour of the theory proposed. Although this may at first sight appear unsatisfactory, yet the same is the case with many other diseases of frequent occurrence, which we are equally at a loss to refer to their primary seat, and can only guess at their probable nature from symptoms which often admit of more than one interpretation; and which are neither uniform nor constant in their occurrence. No difficulty of this kind can generally be complained of in cholera, as its symptoms are, for the most part, sufficiently definite in their character, and regular in the order of their occurrence, as to leave the observer in no doubt respecting what disease the patient is suffering from. This is a point of considerable importance in the pathology of cholera, and contrasts favourably with many other diseases referrible to the nervous system, and more especially with the Protean character of the symptoms in hysteria. It indicates that cholera proceeds from some uniform cause, which, however mysterious it may be in its process, is certain in its results.

The merit of having first directed attention to the sympathetic nervous system, as the probable seat of disorder in cholera, is due to some of the surgeons of Bengal, who seem to have entertained the same view respecting the nature of the disease, as that which it is the object of the present treatise to establish. In the report of the first epidemic of cholera in Bengal, Jameson observes, "the preceding remarks would seem then to warrant a conjecture, that the stomach and small guts, are the primary seat of lesion in cholera. It remains to inquire of what nature this lesion is. By some it has been assumed to consist in a violent affection of the nerves distributed on their inner surface, superinducing great general disturbance of the nervous influence, and universal depression of the system. But, if this were the case, the symptoms in cholera should be similar to those of other disorders manifestly affecting the brain and sensorium by sympathy with the nerves of the stomach. And yet how different are they from the drowsiness, loss of memory and of voice, stupor, and universal convulsions, which always speedily follow the application of narcotic substances to the inner coats of the stomach."[a] The objection here urged against the theory by Jameson, need not deter us in the present day from accepting it. On the contrary, this absence of sympathy between the brain and the stomach, which appears to be considered as the only stumbling-block in the way, will subsequently be found to furnish one of the strongest arguments in its favour.

The opinion thus early and distinctly enunciated in Bengal, was to some extent entertained in the other presidencies of India, on the first invasion of the disease. Scot observes, in the preface to the Madras report, that "the symptoms of cholera may be explained by supposing a diminished energy of the nervous system, but especially of that part of it which supports the vital and natural functions......

---

[a] "Bengal Report on Cholera:" Calcutta, 1820, pp. 81, 82.

The diminished energy, assumed, of the nervous system, accounts for the depressed state of the circulation, and the depraved respiration; and we may suppose that these two functions are so intimately connected, that an irregularity of the one will affect the other. It is not, however, intended here to advance, that the suppression of the circulation, and the depravation of the function of respiration, stand in the relation of cause and effect. It is rather believed that they both are effects of the same cause."[b]

It may probably be supposed, that as Scot did not publish his report on cholera until four years after the appearance of Jameson's report, that this opinion was in some measure borrowed from that of the Bengal surgeons. But so far from such being the case, there is evidence to prove, that, at a very early period (nearly two years before the publication of the Bengal report on cholera), the disease in the Madras territories had been ascribed, not only to an affection of the sympathetic nervous system, but the stomach and intestines had also been referred to as its primary seat. In a letter from Darwar, dated, "6th September, 1818," assistant-surgeon, J. Stone, observes, "Whether it (cholera) is owing to the influence which the morbid affection and diseased state of the nerves of the stomach and intestines have upon the animal economy by sympathy, that the distressing symptoms which mark this disease are induced, I do not feel myself prepared to say, but I am much inclined to give them the credit of it."[c] From the date of this letter, it is probable, that the opinion it contains was the result simply of personal observation of the disease, independent of any intercommunication of ideas on the subject with the surgeons of Bengal.

---

[b] "Madras Report on Cholera:" Madras, 1824. Preface, pp. 36, 37.

The Editor of the "Bombay Reports on Cholera," although he professes to have formed a less decided opinion, may be quoted as partially supporting the same view. He states "that the united testimony of all who have witnessed it (cholera) seems to shew, that there is a *somewhat* which presses heavily and suddenly on the vital functions, and on many occasions, resembles the effects of a poison taken into the stomach or applied to the blood; but whether it acts more immediately upon the circulating system or the nervous, we cannot determine."[d]

Considering the early period at which these opinions were published, before so many false facts, as well as false theories, had been promulgated respecting cholera, it is important to notice this general uniformity of ideas concerning its nature; and although the disease was doubtingly ascribed to an affection of the nervous system, without always signalising the sympathetic as that specially intended, yet it is evident that many observers were induced, from the nature of the attack, to regard this as the most

[c] "Madras Report on Cholera:" Madras, 1824. p. 105.

[d] "Bombay Reports on Cholera:" Bombay, 1819. Preface, p. 31.

probable cause of the disease, and they seem to have looked to it, as the only theory capable of affording anything like a satisfactory explanation of the phenomena. Wherever the disease has since prevailed, the same views have been, to a certain extent, entertained, and though often ridiculed, and, as some may think, refuted, the theory which ascribes cholera to some affection of the sympathetic nervous system, is still the only one capable of satisfactorily explaining its nature.

ON THE PROBABLE SEAT OF THE PRIMARY AFFECTION IN CHOLERA.

HAVING shewn, in my introductory remarks, that cholera has been very early and very generally referred to some affection of the sympathetic nervous system; I would not proceed to enquire in what way the effect is produced; whether it should be considered, as many writers have alleged it to be, the result of a general impression, or whether one organ of the body has more claim than another to priority in the order of attack.

Among the most distinguished of those, who have ascribed cholera to a general impression on the sympathetic nervous system, is Mr. Hamilton Bell,[a] who observes, in his Treatise on Cholera, which is one of the most philosophic works that have been published on the subject, that the disease "cannot be referred to a topical affection of any of the great organs." This opinion is founded on the supposition that ganglia and other nervous centres originate nervous power. If Mr. Hamilton Bell could have ascertained the true source of organic nervous power, he would have completely succeeded in explaining all the phenomena of cholera; but he has failed in doing so, because he regarded "the semi-lunar ganglion and the solar plexus as the principal source of the energy of the sympathetic system."

---

[a] "Treatise on Cholera Asphyxia." Second Edition, 1832.

There is some difficulty in comprehending what is meant by a general impression affecting the sympathetic nervous system,—a difficulty which seems to have been fully appreciated by Bell himself, since he has avoided all reference to the way in which the morbid effect is produced. We can only suppose, that the term "general impression" is intended to express, that the morbid cause, whatever may be its nature, acts through the medium of the skin, each portion of which may be considered as liable to be equally and simultaneously affected. As there is no possibility of directly proving or disproving this hypothesis, it is necessary to have recourse to analogy, and to examine what evidence there is for assuming that the alimentary mucous membrane, to which especially the sympathetic nerve is supposed to be distributed, and from which the peculiar serous discharge takes place in cholera, may be affected through the medium of the skin.

There are cases constantly occurring, in which disease of some portion of the alimentary canal, appears to be the result of a morbid cause affecting the skin. The external application of cold may be cited as a frequent cause of diarrhœa and other disorders of the alimentary canal; but this relation between the cause and the effect of a morbid agency, cannot be satisfactorily applied to cholera; for cold, even if proved to be the cause of disease in such cases, is equally liable to produce disease in other internal organs; whereas the cause which produces cholera, leads always to definite results. A more satisfactory illustration of the nature of the sympathy, subsisting between the skin and the alimentary mucous membrane, is afforded in cases where ulceration of the duodenum results from external burns, and also in those rare cases, where inflammation or other morbid condition of some portion of the alimentary canal, has been observed, in persons struck by lightning, as an occasional sequence to the sore produced by the superficial passage of the electric fluid. A reversed proof of this relation is afforded in those cases of Urticaria, which result from irritation of the alimentary mucous membrane; a frequent consequence, in certain individuals, of eating shell-fish, etc. Lastly, the relation between the skin and the alimentary mucous membrane is manifested occasionally in health, when the third dentition of old age, is accompanied by a renewed growth of hair.

But none of these illustrations are capable of supplying us with any clue to the origin of cholera, or are likely to clear up the mystery which hangs over the nature of the disease. For, as I have before mentioned, cholera must be regarded as the definite result of some powerful agency, acting in a uniform manner; and therefore, neither cold, nor heat, nor electricity, is sufficient to account for it, nor apparently can any other cause, which has been hitherto observed to be capable of disturbing the healthy relation between the skin and the alimentary mucous membrane. We may therefore proceed to consider those theories which refer cholera to some local affection of the sympathetic nervous system, and which may be arranged under three heads:—

1st. Cholera, as the result of structural disease of the semilunar ganglia, solar plexus, and adjoining portions of the sympathetic nervous system.

2nd. Cholera, as the result of functional disorder of the sympathetic nervous system, produced through the medium of the lungs.

3rd. Cholera as the result of functional disorder of the sympathetic nervous system, produced through the medium of the alimentary canal.

I.—*Cholera as the result of structural disease of the semilunar ganglia, solar plexus, and adjoining portions of the sympathetic nervous system.*

The theory which ascribes cholera to structural disease of the central parts of the sympathetic nervous system, appears to have been founded on the opinion of the once-celebrated Dr. Loder, of Moscow, who endeavoured to demonstrate that the disease was, in the first instance, an affection of the sympathetic nervous system, the result of electro-magnetic phenomena; and great efforts were made during the prevalence of the first epidemic of cholera in Russia, in the years 1830 and 1831, to establish a direct connection between the symptoms during life, and certain morbid appearances, which were supposed to be always present, after death from cholera, in these parts of the sympathetic nervous system. The suggestion thrown out by Loder seems to have been received with general attention, and a large number of the post mortem examinations in cases of cholera at this period, not only in Russia, but wherever the disease prevailed in Europe, were made, with the object of ascertaining whether these central parts of the sympathetic system presented any appearance of disease. For although cholera was assumed by some contemporary writers to be due to epilepsy, or other functional disorder of the sympathetic nervous system, yet the prevailing opinion seems to have been, that it was the result of some organic lesion of the sympathetic nerve.

It was at one time believed by many observers, that cholera could be conclusively demonstrated to be the result either of inflammation or congestion of the semilunar ganglia, solar plexus, and other parts of the sympathetic system; and it was proposed, in consequence, to substitute the term Trisplanchnia for that of cholera, as more correctly expressing the nature of the disease. The writings of Scipio Pinel, Foy, Coste, Lowenhayn, Delpech, Lizars, and many others, may be referred to, as illustrating the favourable reception of this theory, on the occasion of the first outbreak of the disease in Europe.

The medical literature, however, of this period, indicates not only that this opinion was warmly advocated, especially in Paris, but it shews also that its popularity was very short-lived; for twelve months had scarcely elapsed, before a complete revulsion of feeling took place; and in consequence apparently of the intemperate zeal with which Prof. Delpech, and others, endeavoured to prove that in all cases of cholera there existed evident structural lesion of some portion of the abdominal sympathetic system, the mere mention of the sympathetic, in connection with cholera, was hardly tolerated at the Parisian Medical Societies; and the re-action soon became so violent, that Rochoux, in his notice of cholera as it occurred at the Bicêtre (1833), states: "l'on serait sifflé à Paris si l'on reproduisait les idées théoriques de l'infortuné Delpech."[b]

It is difficult, in the present day, to estimate the value which should be attached to the statements formerly made respecting the morbid appearances of the sympathetic nervous system, as little or no attention is now bestowed on this subject; and although, during the latter part of the first epidemic of cholera in Europe, subsequent enquirers failed to establish their correctness, it is questionable whether the supporters of this theory altogether deceived themselves, by mistaking post-mortem discoloration, etc., of the parts, for changes occurring during life, or whether, as I am inclined to think may sometimes happen, the duration of the symptoms of cholera is liable, after a certain time, to be attended by, and perhaps to give rise to, evident alteration in these parts of the sympathetic nerve. If such, however, should be the case, it would not assist us in explaining the nature of cholera; for the change so produced would be the effect and not the cause of the disease.

---

[b] "Notice sur le Choléra-Morbus en général, et en particulier sur celui de Bicêtre," Paris, 1833, p. 8.

II.—*Cholera as the result of functional disorder of the sympathetic nervous system, produced through the medium of the lungs.*

This theory appears to have been first distinctly advanced by Mr. Alexander, one of the surgeons of the Madras establishment, who in a Report on Cholera, dated, "Secunderabad, 12th July, 1821," observed that the disease might be defined to be "an affection of the sentient extremities of the pulmonary system of nerves, creating a sympathetic derangement in the nerves having any link of connection with them, and by preventing oxygenation of the blood, causing death."[c]

When the disease first made its appearance in Europe, it was very difficult to say what was the most general or the best supported opinion respecting its nature; but from among the numerous works on cholera published about this time, I may select that by Dr. Keir, who had the opportunity of watching the disease at Moscow, and who seems to have been unbiassed by any previously formed theories on the subject. It was the opinion of Dr. Keir, founded on extensive observation of the disease, that cholera proceeded from miasm, which "seems to contaminate the blood by the organs of respiration."[d]

---

[c] "Madras Report on Cholera," 1824, p. 222.

These opinions were subsequently adopted and enforced by Dr. Copland,[e] who considered cholera to be the result of a morbid impression on the nerves of organic life, produced "by an animal miasm, or effluvium of a peculiar kind, emanating from the bodies of the sick," and which, "being inhaled with the air into the lungs, paralyses those organs, and acts as a poison on the class of nerves which supplies the respiratory, the assimilating, the circulating, and secreting viscera."

This theory has since continued to have numerous and distinguished supporters, and has been strongly advocated by many writers on cholera of the present day, as the only one capable of affording a rational explanation of the principal phenomena of the disease. Of the three important points which appear to be necessarily involved in it, viz., The question of contagion; the state of the lungs; and the condition of the blood, I shall here only refer to the second; for if it can be shewn, that the state of the lungs is opposed to the supposition that these organs are affected, in cholera, to a greater extent, or sooner than the other viscera, it will be sufficient to induce me to reject the theory without further enquiry.

---

[d] "A Treatise on Cholera," Edinburgh, 1832, p. 84.

[e] "Foreign Quarterly Review," Oct. 1831. See also, "Pestilential Cholera; its Nature, Prevention, and Curative Treatment," 1832.

On the condition of the lungs in cholera.

It has been very commonly asserted that the lungs after death from Cholera appear much congested, and so general has been the belief in this statement, that many writers, assuming it to be correct, have, in consequence, ascribed the disease to an arrest of blood in the pulmonary capillaries. But it appears to me, that this statement has been founded on observations which have not been made generally with sufficient exactness; for notwithstanding the contradictory remarks on this subject by different writers, there is sufficient trustworthy evidence to justify the inference, that a congested state of the lungs is not the usual condition of these organs after death occurring in the collapsed stage of the disease; but, on the contrary, that there is a decided deficiency of blood in the proper structure of the lung, and that the apparent accumulation of dark blood, so much dwelt on by some writers, will generally be found to be limited to the great vessels at the roots of the lungs, whilst the pulmonary tissue itself presents an ex-sanguine and collapsed appearance.

In the preface to the Madras Report on Cholera, Scot states that, the lungs were sometimes found "collapsed into an extremely small bulk, and lying in the hollow on each side of the spine, leaving the cavity of the thorax nearly empty. This appearance has been so remarkable as to induce Dr. Pollock, of Her Majesty's 53rd Regiment, to conceive that it could only be produced by the extrication of a gas within the cavity of the pleura, capable of overcoming the atmospheric pressure. It is understood, however, that opportunities were had of piercing the thorax of the dead body under water, and that no gas was extricated. As there appears to have been an absolute vacancy in the cavity of the pleura, that is to say, the lungs did not by any means fill it, it would seem that that viscus had exerted a contractile power, adequate to overcome the pressure of the atmosphere."[f]

When the disease made its first appearance in Europe, the French pathologists were among those who exerted themselves with the greatest zeal to determine its nature, and many of their post mortem examinations deserve to be distinguished for minute accuracy of observation. From the appearances they have recorded, it is evident that pulmonary collapse was a customary condition of the lungs in patients dying of cholera, before reaction had commenced.

---

[f] "Madras Report on Cholera," 1824. Preface, p. 33.

Among the French writers, who may be specially referred to in support of this statement, is Rochoux,[g] who had favourable opportunities for observing cholerà, and who dwells with much emphasis on the ex-sanguine state of the pulmonary tissue. Broussais may be quoted for the same purpose, and his evidence is the more valuable from its being, as many might be disposed to say, opposed to the theory he advanced, of cholera being a disease essentially inflammatory (*eminemment inflammatoire*). "Les poumons (M. Broussais records) sont vides d'air; ils ont perdu beaucoup de leur volume, sans pourtant être aplatis, et présentent en arrière une couleur violette assez foncée. Lorsqu'on les incise, on les trouve sains dans leur tissu, fermes, peu crépitant, trés denses; ils sont plus aérés, si d'anciennes adhérences les maintiennent fixés aux parois de la poitrine."[h]

So also Dr. Payne, in his account of the first outbreak of cholera in New York, in 1832, states, "We frequently find them (the lungs) very much contracted and empty."[i]

But probably the most satisfactory evidence that can be adduced, is that afforded by Dr. Parkes,[k] whose observations on the subject were made with unusual exactness, and are calculated to remove any doubt that may exist respecting the state of the lungs in connection with collapse. Dr. Parkes has succeeded in proving that, in most cases of cholera, the lungs are less crepitant than usual, and that their specific gravity is diminished; showing that there is not only an absence of air, but also of blood, from the proper pulmonary tissue. The diminution of weight, in the case of both lungs, was found by Dr. Parkes to average 20 oz., assuming the healthy standard weight for both lungs in males to be, according to Dr. Clendinning, 46 oz. The extent of the pulmonary collapse was sometimes very considerable; thus, of thirty-nine cases, in which the condition of the lungs was carefully noted by Dr. Parkes, "in fourteen cases, the lungs were completely collapsed, appearing in some cases almost like the lungs of a fœtus. In three cases, they were considerably, and in eight cases they were slightly, collapsed; and in the remaining fourteen cases, the collapse was in some cases altogether, and in other cases, partially prevented by old adhesions." Dr. Parkes states, as the result of this collapsed condition, that "in twenty-four cases, the crepitation was totally abolished; in fifteen cases it was notably diminished in some part of the lung, and in one of these abolished completely in the upper lobes. The want of air was not owing to mechanical impediment, as, on artificial respiration, air passed readily in, distended the before collapsed lung, and partially or wholly restored the crepitation. This," Dr. Parkes proceeds to observe, "I proved by many trials."

---

[g] "Notice sur le Choléra-Morbus, etc.," Paris, 1833, pp. 23, 24.

[h] "Le Choléra-Morbus Epidémique," Second edition, Paris, 1832, p. 60.

[i] Letters on the Cholera Asphyxia," New York, 1832, p. 149.

[k] "Researches into the Pathology and Treatment of the Asiatic or Algide Cholera," 1847.

That this collapsed condition of the lungs in cholera, is independent of any affection of the cerebro-spinal system, may be assumed from the fact, that the mechanical part of respiration is not materially interfered with; for patients, even in advanced stages of the disease, can generally make a tolerably deep inspiration when called upon to do so. Whilst the opinion, that it is due to paralysis of the pulmonary sympathetic nerves, appears to me to be altogether untenable; for the effects of such paralysis would be to cause engorgement of the lungs, and subsequently asphyxia. But as there is no evidence to shew that either engorgement of the lungs, or asphyxia, is the usual or necessary consequence of cholera, the hypothesis that the disease is due to paralysis of the lungs must be rejected.

The true explanation of pulmonary collapse in cholera, is to be sought for elsewhere than in the lungs themselves; for it is an indication of changes resulting from diminution or suspension of the sympathetic nervous influence in other parts of the system; at the same time, that it is a proof of the sympathetic nervous power of the lungs themselves being unaffected. Its occurrence in cholera satisfactorily accounts for all the changes dependant on the function of respiration, and is perfectly consistent with the diminished amount of air inspired, the want of animal heat, and the small amount of carbonic acid eliminated. These conditions in cholera are all necessarily dependant, the one on the other, and, therefore, the existence of pulmonary collapse after death, like the diminished expiration of carbonic acid during life, affords sufficient evidence to enable us to judge of the nature and extent of the morbid changes, which have been effected elsewhere in the system.

Without attempting to enter fully into the nature and extent of those changes which might be grouped together under the title of "The Chemistry of Respiration in Cholera," it will be desirable to notice some of the circumstances affecting the formation of carbonic acid in the system; as the pulmonary collapse in these cases, is caused by the amount of carbonic acid to be eliminated being reduced, whilst the nervous power of the lungs continues the same, or nearly so. The amount of carbonic acid in the air expired by cholera patients is, therefore, capable of furnishing us with the means of ascertaining the extent to which the functions of other parts of the system are affected, and in this sense, to use the appropriate simile of Dr. O'Shaughnessy, it may be compared "to the hands (of a time-piece), by whose motions is estimated the regularity or aberration of the internal machinery."[1]

From the researches of Dr. John Davy, it would appear that the amount of carbonic acid in the air expired by a cholera patient, may be reduced to one-third of the usual quantity. This enables us to account for the great general reduction of animal heat, the formation of which, in the system, it would be inconsistent to suppose could continue uninterrupted or undiminished, since there is usually no obstacle in cholera to the exit of carbonic acid by the lungs. On the contrary, it is probable, that in advanced stages of cholera, when the circulation in the extremities is arrested, no carbonic acid is developed in those parts, to which the supply of blood has been stopped; and that by far the greater portion of the carbonic acid which is eliminated by the lungs, is derived from the epigastric and adjoining regions, the temperature of which is usually increased in cholera, and has been observed as high, in some cases, as 106° Fahr. or even 112° Fahr.[m] As the animal heat in this situation is persistent, and even increased in cholera, whilst it fails in distant parts of the system, it may be admitted as an established fact, that if cholera is dependent on an affection of the sympathetic nervous system, that the seat of the disease should be referred to that part of the system in which the development of animal heat is the greatest.

---

[1] "Report on the Chemical Pathology of the Malignant Cholera," 1832, p. 41.

As the evolution of carbonic acid in the lungs is dependent on the law which regulates the diffusion of gases through moist membranes, it is evident, that if the entrance of atmospheric air to the lungs is unimpeded, the amount of carbonic acid disengaged must depend on the amount not only of venous blood sent to the lungs, but also on the amount of carbonic acid which that blood contains; and, as both the absolute amount of blood sent to the lungs, and the relative proportion of carbonic acid it contains, appear to be diminished in cholera, whilst the nervous power of the lungs remains unaffected, the attraction for venous blood is greater than the supply, and the lungs in consequence collapse. Pulmonary collapse affords, therefore, in these cases anatomical proof, that the lungs during life continued to perform their office well, and that the blood sent to them was not only not surcharged with carbonic acid, but contained probably less than the usual amount; for had the reverse of this been the case, in either instance, the circulation through the lungs, and, consequently the respiration would have been increased, and pulmonary collapse could not have occurred.

---

[m] Dr. Payne's "Letters on the Cholera Asphyxia," 1832, p. 116.

The diminished respiration in cholera is to be regarded as a fortunate as well as a necessary condition, during the continuance of cholera collapse; for if the quantity of oxygen admitted into the system, were greater than could be disposed of in the course of the circulation, the balance of the organic functions would be still further disturbed, and greater mischief would ensue. Hence the inhalation of oxygen and nitrous oxyde gases, which have both been recommended and had recourse to in cholera, are objectionable; and, notwithstanding any temporary improvement in particular symptoms, which they may appear at first to produce, they are both liable to give rise to serious complications in the disease; and the objection to their use is greater in proportion to the duration of the cholera collapse. For the observations which I have made respecting the condition of the lungs are not intended to preclude the idea, that in those cases of cholera, in which the blood becomes very thick and glutinous, the carbonic acid developed in it, is always being removed by the blood being attracted to the lungs. On the contrary, it is probable, that in many cases of cholera, the great change effected in the consistence of the blood, is sufficient to a certain extent to overcome the attraction in the lungs, and hence the arrest of blood in the portal circulation, may be due more to the physical than the chemical constitution of the blood. Consequently, the pulmonary circulation may sometimes fail, not so much from the blood, in other parts of the body, being deficient in carbonic acid, as from the want of a sufficient quantity of water in that blood, to allow of its circulation to the lungs being continued. But this condition of the system is only one of the sequels of cholera, and not a necessary part of the disease.

III.—*Cholera as the result of functional disorder of the sympathetic nervous system, produced through the medium of the alimentary canal.*

The theory which ascribes cholera to an impression on the sympathetic nervous system, conveyed through the medium of the stomach or intestines, has been referred to in the short general review of the history of the sympathetic nervous theories of cholera, as one of the earliest opinions entertained respecting the nature of the disease; and although the Bengal surgeons, to whom I have already alluded, did not attempt to explain the phenomena of the disease, in accordance with the theory proposed, it is but just to acknowledge their claim to the merit of having first made the suggestion. Since their time, the theory has been on several occasions advanced and rejected, and has undergone some modifications which relate chiefly to the exact situation of the primary affection; for whilst some of the supporters of the theory have adopted a restricted view of the primary operation of the morbid cause, and have thought that in the first instance, it affects only the stomach, or the duodenum; others have been inclined to consider, that both the stomach and upper portion of the intestinal canal are simultaneously affected. It would, probably, not be attended with much practical advantage to determine which of these opinions may be correct, as such knowledge could have little or no influence on the treatment of the disease; but I may here incidentally state, that from my own observations of the disease, extending over many years, I am disposed to consider the stomach as the centre of the affection in cholera; and the following remarks will contain my chief reasons for entertaining this opinion.

The arguments by which it is supported are derived from two sources: 1st, the general relation between cholera and other disorders, which admit of being directly referred to an affection of the sympathetic nervous system, produced through the medium of the stomach: and 2nd, the analysis of particular phenomena in cholera.

I.—*The general relation between cholera and other disorders which admit of being directly referred to an affection of the sympathetic nervous system, produced through the medium of the stomach.*

It has been so much the custom to speak of cholera as a disease *sui generis*, and altogether unlike any other disease of which the human body appears to be susceptible, that it may be thought by many to be impossible to establish an analogy between cholera and other diseases. Its sudden and unaccountable origin, the peculiar character of many of its symptoms, and the apparently negative evidence of the post-mortem examinations, have been supposed to offer so many obstacles to a satisfactory explanation of its nature, as to preclude the possibility of classifying it with other diseases. There are, however, several morbid affections of the body, which appear to me to offer some decided analogies with cholera, and which have the advantage of being less obscure in their origin. These I propose to include with cholera, under the head of "Disorders of the Sympathetic Nervous System, depending on diminution or arrest of its functions, and produced chiefly through the medium of the stomach." One or more of the symptoms which are present in cholera, may be either absent or unnoticed in some of these cases; but the general sameness of the essential symptoms is, in each case, sufficient to establish a close relationship between them. In all of them, the stomach appears to receive a shock, either from direct violence or other cause acting suddenly, and which may be either directly or indirectly communicated to it.

As a familiar instance of the stomach receiving a shock, from *external* violence, I may refer to the effects following a severe blow upon the epigastrium. On some occasions, when the blow has been violent and unexpected, death has almost immediately followed, and the post mortem examinations of the body have not unfrequently failed, in these cases, to reveal any organic lesion. In the suddenness of the effect produced, and in the absence of any *evident* organic lesion, these cases bear a close resemblance to those in which death results from concussion of the brain; and it is deserving of especial notice, that in each case, the violence is applied near the centres of the two nervous systems. This will be more fully considered, when I speak of the probable use of the ganglia of the sympathetic; but I may here remark, that the violence in these cases, applied apparently to opposite parts of the system, tends always to one result, and proves fatal in one way, viz. by its effect on the circulation of the blood. A parallel instance to the suddenly fatal effects following blows upon the epigastrium, has been occasionally observed in the cholera of India and elsewhere; when individuals attacked by the disease have suddenly become giddy, fallen down, and, after one or two slight efforts to vomit, have expired in a few minutes. In many cases also of cholera occurring in tropical climates, without such rapidly fatal consequences ensuing, the shock to the sympathetic nervous system appears to be often greater than in colder countries; and, consequently, the impression made on the nerves in the stomach is more liable to be transmitted to the cerebro-spinal system. Hence the greater frequency of syncope, tetanus, and other affections of the cerebro-spinal system, in persons attacked with cholera in India, compared with what is observed in this country.

The occasionally fatal consequences of drinking cold water when the body is heated, furnish a corresponding illustration of the stomach receiving a shock, from an *internal* cause, unconnected with any *evident* organic lesion. The death on these occasions, when it takes place suddenly, usually resembles very closely that which results from blows, etc., upon the epigastrium. One of the most instructive cases of this kind, that I am acquainted with, is contained in Dr. Currie's Paper on the "Effects of drinking Cold Water." The patient was "a young man, who had been engaged a long time in a most severe match at fives. After it was over, he sat down on the ground, panting for breath, and covered with profuse perspiration. In this state, he called to a servant to bring him a pitcher of cold water just drawn from a pump in sight. He held it in his hand for some minutes, but put it to his head as soon as he had recovered his breath, and drank a large quantity at once. He laid his hand on his stomach, and bent forwards, his countenance became pale, his breath laborious, and in a few minutes he expired."[l] Dr. Rush, of Philadelphia, whose experience in these cases is considerable, states, that in the majority of them, the patients "are seized with acute spasms in the breast and stomach. These spasms are so painful as to produce syncope, and even asphyxia. They are sometimes of the tonic, but more frequently of the clonic kind. In the intervals of the spasms the patient appears to be perfectly well. The intervals between each spasm become longer or shorter, as the disease tends to life or death."[m] The analogy derived from the primary effects of drinking cold water, in these cases, is supported by the secondary consequences which are sometimes observed to follow, in those cases in which the immediate shock has been recovered from, and which consequences are very similar to what occur frequently as sequels to cholera.[n]

---

[l] "Medical Reports," 3rd edit., 1804; vol. i., p. 98.

[m] "Medical Enquiries and Observations," 2nd edit., Philadelphia, 1805; vol. i., p. 18.

[n] "Persons who have recovered from the immediate danger which attends this disease (caused by drinking cold water), are sometimes

The effects of over-distension of the stomach, from food or gas, afford another illustration of the stomach receiving a shock, from an *internal* cause, which may prove fatal in the same way, by inducing collapse, without leaving any evidence of direct injury. It is true, that in some cases the over-distension gives rise to rupture of one or more of the coats of the stomach; but this is not necessarily the case, for death has been sometimes observed to occur without any lesion of continuity. Dr. Christison, who seems to have fully appreciated the importance of this fact, remarks, that death from distension of the stomach "is the consequence, not always of apoplexy, but sometimes of an impression on the stomach itself";[o] and he refers to two cases, which confirm this opinion. When the shock has not been so severe as to cause death, still the symptoms which sometimes result from over-distension are very alarming, as the nervous power appears to be suspended for some hours. Dr. Wood[p] refers to the case of a gentleman, in Philadelphia, who narrowly escaped interment, when in a state of protracted syncope, which was believed to have resulted from the eating of fresh and badly baked bread; and I have lately had the opportunity of observing a somewhat similar case, in which prolonged syncope was caused by excessive feasting. The over-

---

affected, after it, by inflammation and obstructions in the breast or liver." Ibid. vol. i., p. 186.

[o] "A Treatise on Poisons," etc., 4th ed., Edinburgh, 1845; p. 115.

[p] "A Treatise on the Practice of Medicine," 4th ed., vol. ii. p. 204.

distension of the stomach in such cases, must be inferred, however, to act not only on the sympathetic, but also on the cerebro-spinal nervous system. This is evident from what has been observed in other cases of a similar character, where the distension has been followed by symptoms less sudden and less intense. The well known consequences of over-feeding in arresting the function of digestion, and the mischievous effects of giving immoderate quantities of fluid after an emetic, from a mistaken idea of aiding its action, shew that over-distension of the stomach acts by arresting all its nervous power. For, as Huxham, in some judicious remarks on emetics, forcibly observes, "a deluge of drink is often so far from promoting vomiting, that by over-distending the stomach, it quite destroys its power of action."[q] The effect produced in these cases must therefore be regarded as the result of something more than the impression made on the sympathetic nerve; for the sympathetic cannot be considered as the direct medium through which either sensation or muscular motion in the stomach is produced. On the contrary, the action of vomiting appears to be almost entirely dependent on the pneumogastric nerves, and when these are paralysed, the stomach ceases to contract.[r] This fact has been often taken advantage of, in the treatment of cholera, when a stimulating emetic, such as mustard, has been given for the purpose of restoring the action of vomiting, in cases where its suspension appears to be owing to want of power on the part of the stomach to expel its contents.

---

[q] "Observations on the Air and Epidemic Diseases, from the year 1728 to 1737, inclusive". 1777; vol. i., p. 27.

[r] "Animals in which both pneumogastrics are simultaneously cut, die almost immediately, when they are permitted to eat as much as they please; for, the contractility of the stomach and œsophagus

In all the foregoing illustrations the morbid cause can be referred directly to the stomach, but it can only be assumed that the effect produced is the result of the local mischief, for the stomach after death, as already stated, seldom presents any trace of injury. In this respect the cases closely resemble cholera, and it would be equally difficult in them, as it is in cholera, to locate the affection, were it not that the stomach can be shewn to be so clearly the organ primarily attacked; and that the same causes fail to produce the same effects when applied to the other abdominal viscera. Blows equal in violence to those which induce fatal collapse, when applied to the epigastric region, are not followed by the same consequences when applied elsewhere on the abdomen, unless rupture of some organ occurs; and even then the patient often survives for a long time after the accident, and dies from the remote effects of the injury, or from other causes. This is especially observable in those cases where the alimentary canal has escaped injury. Mr. Erichsen relates the following instructive case:—"A patient was admitted last year under

---

being destroyed, the food, after having filled the stomach, distends the œsophagus, and passes from it into the larynx."—Cruvelhier's "Descriptive Anatomy," vol. ii., p.1138 (Library of Med.).

my care into University College Hospital, who had been crushed between the buffers of two railway-carriages. He was collapsed and apparently moribund, but rallied in a few hours. Two days after the accident, great pain and tenderness in the right hypochondrium were complained of, and dulness on percussion was found to extend as low as the umbilicus. He became jaundiced, and there were symptoms of low peritonitis. These were followed by great swelling of the abdomen, which became tympanitic. The peritonitis continued, and symptoms of intestinal obstruction came on; the dulness increasing with fluctuation in the flanks. He died on the 16th day after the accident, and on examination no less than two hundred and forty ounces of bilious fluid, mixed with flakes of lymph, were found in the abdominal cavity. The obstruction appearing to be dependent on the pressure of this effusion, and on the matting together of the intestines by lymph. There was a large rent found in the thick border of the liver, which was apparently beginning to cicatrise."[*] Cases of a somewhat similar description, in which the spleen, the kidney, etc., have been lacerated by violence, have been occasionally observed to recover, or have proved fatal only after the lapse of weeks, or even months, from the receipt of the injury. So, also, in cases of over distension of the other abdominal viscera, as of the intestines, or urinary bladder, such as occurs in tympanitis, or prolonged suppression of urine, the distension may proceed to almost any extent, without producing corresponding symptoms of collapse. With regard to the bladder, prolonged retention of urine is well known to lead to a temporary diminution or even arrest of its nervous power, in consequence of which, that viscus ceases to possess the power of expelling its contents; but the other viscera of the abdomen are not necessarily involved in the mischief.

---

[*] "The Science and Art of Surgery," 1853, p. 304.

These cases, however, as already mentioned, tend only to prove that fatal collapse may result from causes acting suddenly on the stomach itself, without producing any organic change of structure in the part. The suddenness with which death generally takes place, may be said to prevent the occurrence of certain symptoms which might strengthen the resemblance to cholera. Hence, to complete the chain of evidence, it is necessary to prove, that a closer relationship would be established between cholera and these allied affections of the nervous system, if sufficient time were allowed for their development. Something more, however, than time seems to be required to effect this; for, in the preceding class of cases, it is often observable that if death does not occur quickly, the first effects of the shock pass off, and the patient recovers without any further unfavourable symptoms. To ensure, therefore, this closer resemblance between the symptoms in such cases and in cholera, two conditions appear to be necessary: 1st., that the shock to the gastric nerves should not be so intense as to prove directly fatal; and 2nd., that the morbid cause, whatever may be its nature, should be more persistent in its operation. The only cases except cholera in which these conditions appear to be fulfilled are spontaneous perforation of the stomach. The resemblance between the symptoms in these cases and in cholera, is frequently so close, that during the prevalence of the latter disease, cases of perforating ulcer of the stomach are not unlikely to be confounded with it, and there are instances on record in which this mistake has occurred. In the following parallel between cholera and perforating ulcer of the stomach, it will be seen that most of the essential phenomena are the same; some less constant or doubtful points of resemblance, which have been excluded from this comparison, will be subsequently alluded to.

CHOLERA.	PERFORATING ULCER OF THE STOMACH.
1. Average duration of life after the attack, in fatal cases, about 18 hours.	1. Average duration of life after the attack, in fatal cases, about 18 hours.
2. Mental faculties unaffected up to the time of death, except when syncope occurs from the suddenness and intensity of the attack.	2. Mental faculties unaffected up to the time of death, except when syncope occurs, from the suddenness and intensity of the attack.
3. Skin cold; perspiration sometimes profuse; sensibility of skin persistent.	3. Skin cold; perspiration sometimes profuse; sensibility of skin persistent.
4. Face pale and sunk; countenance anxious, often cadaverous.	4. Face pale and sunk; countenance anxious, often cadaverous.

Cholera.	Perforating Ulcer of the Stomach.
5. Præcordial oppression.	5. Præcordial oppression.
6. Severe burning pain at the epigastrium.	6. Severe burning pain at the epigastrium.
7. Restlessness.	7. Restlessness.
8. Urgent thirst, and desire for cold drinks.	8. Urgent thirst, and desire for cold drinks.
9. Pulse at first small and quick: subsequently becomes imperceptible at the wrists.	9. Pulse at first small and quick; sometimes becoming imperceptible at the wrists, some hours before death.
10. Respiration diminished.	10. Respiration diminished.
11. Voice feeble and altered.	11. Voice feeble and altered.
12. Vomiting.	12. Vomiting in some cases only; but in all a great desire to vomit, without frequently the ability to do so.
13. Suppression of urine.	13. Suppression of urine.

The parallel which has been drawn between these two affections might be still further extended, as there are other points of resemblance which are not included in the preceding list; chiefly, as I have before stated, from being less constant in their occurrence. Cramps, which affect both the extremities and the abdomen in cholera, are generally limited to the abdomen in cases of perforating ulcer of the stomach. With reference to the post-mortem examinations in these cases, except the lesion of continuity and consequent peritonitis, there are the same negative appearances in the viscera of the head, the chest, and the abdomen, after death from perforating ulcer of the stomach, as are generally met with after death from cholera. Moreover, the peritonitis resulting from perforating ulcer of the stomach is sometimes very partial, and appears to be but little concerned in causing death; for, in some cases which have come under my own observation, the extent and severity of the inflammation has appeared to be inversely to the quickness with which death has occurred. It is important to notice in connection with the suppression of urine, which has usually been regarded as peculiarly characteristic of cholera, but which, as I shall subsequently have occasion to point out, is almost equally constant in cases of perforating ulcer of the stomach, that the urinary bladder is also in these cases, as in cholera, generally found to be empty and contracted. Lastly, there is one other point of connection between cholera and perforating ulcer of the stomach, which I had hoped to have been able satisfactorily to have established, but which I can hardly venture at present to say that I have succeeding in doing. This is the occurrence of a serous flux in cases of perforating ulcer of the stomach, which appears to be identical in character with that which occurs in cholera. In three out of seven cases of perforating ulcer of the stomach, which I have attended, I have noticed the presence in the stomach and cavity of the abdomen, not only of an unusually large amount of fluid, but of fluid possessing the gruel-like appearance which is so peculiarly characteristic of cholera, and which could hardly be supposed to be the result of the peritonitis, or of anything that the patient had been allowed to swallow. I must forbear, however, to insist on this argument; for, although further observation may confirm it, there is not as yet sufficient proof in its favour to allow of its being received as correct.

The importance of traumatic peritonitis in these cases has been probably overrated; for death often appears to occur more from the effects of the injury on the nervous system than from the inflammation produced. This is sometimes shown to be the case in spontaneous rupture of the stomach, an accident very similar in its effects to perforating ulcer of the stomach, and which usually causes death in the same way by collapse, after an interval of some hours; but it has occasionally been observed to prove immediately fatal, as in the following case:—"A coal-heaver, aged about fifty, whilst stooping in the act of lifting some coals, placed his hand suddenly on the pit of the stomach, and complained of severe pain in that situation: this was immediately succeeded by two deep sighs, when he dropped down and expired."[t] An opening, sufficiently large to admit the thumb, was discovered on the posterior surface of the large extremity of the stomach, from which part of its contents had escaped. Some doubt prevails respecting the exact nature of the lesion in this case, whether it should be considered as rupture, or as perforation from ulceration; it may probably, notwithstanding the apparent absence of any signs of ulceration in the stomach, have been the latter. The case, however, is instructive, as it proves that death may be the result of shock to the gastric nerves, since there was no time for inflammation to be developed.

---

[t] "London Medical Repository," vol. xvii. pp. 108, 109.

The secondary position of traumatic peritonitis, as a cause of death in injuries of the stomach is still more forcibly illustrated in those cases where the stomach has been lacerated by wounds inflicted through the parietes of the abdomen. The well-known case of the Canadian, St. Martin, who was wounded in the stomach in 1822, and many others, who have recovered after very severe wounds in the stomach,[a] show that the great source of danger, is not so much the peritonitis, as the shock to the gastric nerves; for the first effects of the injury in these cases being felt by the integuments of the abdomen, the shock to the gastric nervous system is diminished, from its being, so to speak, prepared for it; and, consequently, there is a greater probability of recovery in these cases, than where the stomach itself receives, as it were, unprepared, the first effects of the injury. Patients have, indeed, been known to recover, even from the effects of perforating ulcer of the stomach, but the extreme rarity of such cases is a sufficient proof of the greater danger which attends them.

---

[a] "A Letter from the Rev. Mr. James Field, Rector of St. John's, in Antigua, concerning two Cases of Wounds in the Stomach, to Mr. John Douglas, Surgeon, F.R.S."—Phil. Trans. 1722, pp. 78, 79.

Before concluding this part of my subject, it is necessary to notice, that, although military surgeons have generally agreed in considering wounds of the stomach as more serious than wounds of the other abdominal viscera, and that the shock to the system, in such cases, is greater; yet the explanation given by some of them differs from that which is here suggested. Hennen, for instance, supposes that the shock to the system, in gun-shot wounds, etc., of the abdomen, is due to the impression being transmitted to the spinal cord. This hypothesis of one of the most distinguished writers on military surgery, cannot be regarded as satisfactory; for the only way in which the impression could be supposed to act, in this case, would be by causing concussion of the spinal cord, and temporary paraplegia. But this result is not observed to follow as a consequence of shock; for if the impression is not so intense as to cause sudden death, or syncope, the functions of the spinal nervous system are not apparently affected; since the patient continues to retain power over the voluntary muscles, up to the period of death, even in those cases where he is said to die from the "pure effects of shock," without any evidence of organic lesion being apparent in any of the viscera.

II.—*The analysis of particular phenomena in cholera.*

Before entering upon the consideration of this, the most important part of my subject, I must acknowledge that I feel some hesitation in carrying out my intention of restricting the size of the work; since the magnitude of the subject, both with regard to the interests it involves and the mysteries it is capable of unfolding, renders it difficult for me to make others comprehend, in a few remarks, all that I would wish to communicate respecting it. It so often happens, that writers in trying to be concise, succeed only in being obscure, that I fear lest the same misfortune should happen to myself; and I shall therefore not attempt to give an analysis of all the phenomena, but shall select only a few of the more prominent, which, if fully analysed and rightly understood, will be sufficient to explain the nature of the disease. The condition of the respiratory organs, with the function of respiration, has been already considered in connection with that theory which refers the disease to the lungs; and it was then observed, that the evidence they afforded was favourable to the supposition of the stomach being the primary seat of cholera. The phenomena I now propose to analyse may be grouped under four heads:—

1. The suppression of urine.
2. The altered condition of the blood.

3. The flux from the stomach and bowels.
4. The failure of the circulation.

1. *The suppression of urine.*

Suppression of urine is one of the symptoms which has generally been thought to be characteristic of cholera; and it is so constantly observed in connection with this disease, that it may be reasonably considered to be an essential constituent in the phenomena of the collapse. The alleged infrequency of its occurrence in other diseases, and the supposition that it can hardly occur, except in cholera, without inducing symptoms indicative of uræmic poisoning, have invested it with a peculiar significance; and without wishing to over-rate its importance, I may remark, that on a correct interpretation of this symptom will depend, in a great measure, our knowledge of the disease.

When the first generally acknowledged outbreak of cholera occurred in India, in the year 1817, the fact that the urine was suppressed, appears to have been altogether overlooked; and even when it was subsequently ascertained that the patients passed no urine, it was thought to be due to retention instead of suppression, and a catheter was not unfrequently introduced into the bladder, with a vain hope of relieving it. The true characters, as well as the constancy of the symptoms, were however soon recognised, together with the circumstance, that even protracted suppression of urine might often occur without being followed by coma or any head symptoms. This was an observation altogether new to most observers of the disease, who appear to have been at a loss to account for so unexpected a phenomenon. The earlier writers on cholera have, consequently, for the most part restricted themselves to mentioning that the urine was suppressed, without attempting to offer any explanation of so unusual a symptom, or the still more remarkable fact, of the absence of the usual consequences of such suppression on the brain. Whilst later writers, who have attempted to explain this symptom on other hypotheses than those connected with the sympathetic nervous system, have either failed in their arguments, or have attempted to shew that the urine is not, strictly speaking, suppressed, but that its constituents find an exit from the body by some other channel; in consequence of which, uræmic poisoning is prevented.

Annesley appears to have been the first to suggest that the watery part of the urine might be got rid of in cholera, by a vicarious discharge; but he does not inform us in what way the peculiar solid constituents of the urine are disposed of. He limits himself to stating that, "the watery part of the secreting fluid, usually secreted by the kidneys, is completely drained off by the diseased action of the mucous surface of the digestive canal."[a] This fails to account for the absence of urea from the system, unless the term, "watery part of the circulating fluid," is meant to include the constituents of the urine usually held in solution, which can hardly be the meaning intended to be conveyed by Annesley, as he appears to consider that the drain from the mucous surface of the alimentary canal is analogous, as regards the functions of the kidneys, to what is witnessed in ascites, where the fluid discharged from the serous surface, may sufficiently account for the deficiency of the watery part of the urine, but does not carry with it any of the urea.

---

[a] "Sketches of the most prevalent diseases of India," 1825, p. 33.

The suggestion thrown out by Annesley, has been had recourse to by many subsequent writers, who have applied it, not only to account for the absence of the watery, but also of the solid constituents of the urine, which they have imagined to be got rid of, by the alimentary mucous membrane performing vicariously the office of the kidneys. Thus Dr. Watson suggests, that the urea "is drained off from the blood, in the enormous and unnatural flux from the stomach and bowels."[b] The same hypothesis has been still more fully adopted by Dr. Wood, who supposes that the uræmic poisoning is prevented by the establishment of a vicarious discharge from the skin and mucous membrane. In reference to this point, Dr. Wood observes, that "in some instances, too, other organs appear to take on a vicarious office, and to relieve the system by throwing off the urea. Thus, in epidemic cholera, in which the urinary secretion is sometimes entirely arrested, the brain remains remarkably clear, in consequence, no doubt, of the copious discharges from the skin and alimentary mucous membrane."[c]

---

[b] "Lectures on the Principles and Practice of Medicine," third edition, 1848, vol. ii. p. 595.

The hypothesis here advanced to explain the absence of the urinary secretion in cholera, has, however, been had recourse to before cholera was known, to account for the same symptom, which has been alleged to occur in certain rare forms of hysteria, in which the patients are stated not to have passed any urine by the appropriate passages, for periods varying from a few days to months and even years. As I shall have occasion, in a subsequent reference to this point, to consider the subject of vicarious secretion more fully, and to examine the evidence on which it rests, I would merely observe here, that late researches into the chemical pathology of cholera tend not only to disprove this hypothesis, but to confirm the opinion I have adopted respecting the cause of the suppression of urine in cholera; and, as I shall subsequently endeavour to prove, in some other allied states of the system, in which the same symptom occurs.

In the following remarks on the pathology of suppression of urine in cholera, I propose to consider; (*a*), the probable cause of the appearance usually presented by the urinary organs, after death occurring in the collapsed stage of the disease; and (*b*), the extent to which the formation of urea in the blood is affected, after its elimination by the kidneys is arrested.

---

[c] "A Treatise on the Practice of Medicine," fourth edition, Philadelphia, 1855, vol. ii. p. 599.

(*a*). *The probable cause of the appearance usually presented by the urinary organs, after death occurring in the collapsed stage of the disease.*

In the early histories of cholera, we find that it was soon ascertained, that the appearances presented by the urinary organs were almost uniformly the same, and incapable in themselves of affording any explanation of the cause of the suppression of urine during life. The importance of this observation was fully appreciated, whilst at the same time it contributed to increase the difficulty of accounting for the nature of the disease itself. Thus Annesley remarks, "that the kidneys were generally of a healthy structure, and presented not any organic derangement, which could explain the complete interruption which their functions had experienced in the course of the disease."[d] Nearly all subsequent writers on the subject have confirmed the correctness of this statement. In like manner, the urinary bladder was found to be so constantly empty and contracted, that this appearance soon came to be regarded as a necessary condition of the organ after death from cholera collapse. With reference to the condition of the kidneys, it is only necessary to allude to the contrast they present in this disease, to what is usually observed in those cases of suppression of urine, in which death has resulted from the urea being retained in the blood, to shew that the cause of the suppression, in these two cases, must be essentially different. With regard to the empty and contracted state of the bladder, it is probable that such is the natural condition of the viscus during life, when it has ceased to retain urine; and it is consequently not unusual to meet with it after death from other causes, and independent of any disease of the urinary organs.[*]

---

[d] "Sketches of the most prevalent Diseases of India," 1825, p. 125.

As the kidneys do not present in cholera any appearances which may not be met with after death from other diseases, in which no symptoms affecting the secretion of urine have been witnessed during life, and which may not, so far as their apparent structure is concerned, be regarded as consistent with health, it is necessary to seek elsewhere for an explanation of that change by which their secreting function has been arrested. The nature of the change, under these circumstances, cannot admit of direct proof; and from the imperfect knowledge we as yet possess of the forces which regulate organic life, we can only, for the present, assume that the effect is produced through the medium of the nervous system; and this supposition is supported by what has been observed to occur in other diseases, in which, like cholera, suppression of urine has been witnessed during life, with an empty and contracted state of the bladder after death, without any organic change in the structure of the kidneys capable of accounting for such phenomena. By a comparison with these cases, the negative evidence furnished by the state of the urinary organs in cholera, and which has hitherto possessed but little value, will be found to acquire great importance, and will contribute to complete the chain of reasoning, which has ascribed the cause of cholera to an impression on the central portions of the sympathetic nervous system acting through the medium of the stomach itself.

---

[*] "The bladder is also found contracted to such a degree as hardly to have any cavity. This is sometimes not to be considered as a disease, but simply as having arisen from a very strong action of the muscular coat of the bladder previously to death."—Works of M. Baillie, M.D, by Wardrop, vol. ii. p. 279.

The only cases in which, at present, I have observed this form of suppression of urine to occur, are those in which death has resulted from perforating ulcer of the stomach. In these cases, during the continuance of collapse, suppression of urine, independent of organic disease of the kidneys, is as constant a symptom as in cholera; and as the nature of the collapse in this disease admits of being more readily accounted for than that of cholera, it will consequently be more easy to explain the cause of the urine being suppressed in cholera, when its suppression in these cases has been explained.

The extent to which the urine is suppressed in cases of perforating ulcer of the stomach, has only been ascertained so far as its elimination by the kidneys is arrested. Whether the formation of the elements of the urine in the blood continues in these cases or not, there has been no opportunity of judging, as no analysis of the blood has been made; but taking the fact of the urine being suppressed, with the co-existence of symptoms of collapse, similar to those which are familiar to us in cholera, I think it is allowable to assume that the formation in the blood of urea, and other essential elements of the urine, is arrested during the continuance of the collapse in these cases, as completely as can be shewn to be usual in cholera.

I am not able to speak decidedly with reference to the extent of the suppression of urine during life, or the precise appearance of the urinary organs after death, in some of my earlier cases of perforating ulcer of the stomach, as my researches into the pathology of this disease were not at first directed to elucidate the connection, which I have since been enabled to establish, between some of its phenomena and those of cholera; but since my attention, during the last few years, has been more particularly directed to confirm the truth of this statement, I have found that the suppression of urine during life, and the appearance of the urinary organs after death, have been sufficiently constant and uniform to justify the inference, that the cause of the suppression of urine in perforating ulcer of the stomach has been similar to that in cholera; and, indeed, more clearly referrible to the shock communicated to the central portions of the sympathetic nervous system through the medium of the stomach. For in perforating ulcer of the stomach, there is ample evidence to prove that the shock is the result of direct injury, whilst in cholera, owing to the disease resulting from some cause at present unknown, it can only be inferred to act in the same way, by its producing a corresponding effect on the system.

The evidence I am able to adduce from my own researches into the pathology of perforating ulcers of the stomach, can receive but little support from the observations of others; for in most of the cases of this description, hitherto recorded of spontaneous perforation of the stomach, the state of the urinary organs is either not referred to, or it is merely remarked, that in common with some of the other abdominal viscera they present no abnormal appearance. In the elaborate treatise on perforation of the stomach, by M. Lefévre,[f] suppression of urine is only mentioned in one case, and then not as a necessary consequence of the lesion, or as being of any value as a diagnostic sign. There are, however, a few cases recorded by other writers, in which the condition of the urinary organs is specially mentioned. In a case of cancerous perforation of the stomach in a man, aged twenty-eight years, related by M. Cazeneuve,[g] the urine was suppressed, and a catheter was introduced into the bladder, but only about two spoonfuls of urine obtained. It was observed by M. Cazeneuve that the catheter could not be turned about in the bladder, owing to the very strong contraction of that organ. In the post mortem examination, the kidneys were found to present no abnormal appearance, and the bladder was contracted and hid behind the pubes. In a case observed by Dr. (afterwards Sir Philip) Crampton,[h] of perforating ulcer of the stomach, in a lady, aged 29 years, death occurred from collapse twelve hours after the accident. The state of the urinary secretion during life is not mentioned; but it is stated, that, after death, "the urinary bladder appeared empty and contracted."

---

[f] "Archives Générales de Med.," Août et Septembre, 1842.

[g] "Gaz. Med. de Paris," 1838.

On referring to the histories of a large number of cases of spontaneous perforation of the stomach, I can find little or no evidence to prove, that the condition of the urinary organs was different from that which has been stated above; and generally speaking, when any allusion has been made to the kidneys in these cases, it is simply stated, that their appearance was healthy. I must, however, admit that the number of cases of this description, in which the condition either of the urinary organs, or their secretion, has been noted, is at present very small, and probably not sufficient to establish the correctness of my opinions on this subject. Further observations may, and, as I believe, will, have the effect of removing this objection, and lead to the accumulation of evidence which may be received as conclusive.

---

[h] "Med. Chirurg. Trans." 1817.

If from spontaneous perforation of the stomach, we pass to similar affections of the lower portions of the alimentary canal, we shall find usually the same omissions with regard to the condition of the urinary organs, till we come to those unfrequent accidents which occur in the course of typhoid fever, where generally the ilium, near the ilio-cæcal valve is the seat of the lesion. In Dr. Stokes' paper on "Peritonitis from perforation of the serous membrane,"[1] suppression of urine is mentioned as having been noticed in three cases of perforation of the ilium; in one of them, that of a patient who appeared to suffer from retention of urine, a catheter was passed, but only a few drops of turbid and high-coloured urine were withdrawn; after death, the peritoneal surface of the bladder was found to be highly inflamed. Dr. Stokes refers the suppression of urine in these cases, to the urinary organs being irritated from contiguity with the seat of perforation. This is no doubt partly the cause of the suppression; which appears to partake, in some measure, of the nature of strangury, but it does not completely account for it. For if acute inflammation of the peritoneum were the cause of the suppression of urine in these cases, we should expect to meet with the same symptom equally often in idiopathic peritonitis, but this is not the case; for although the amount of the urinary secretion is much diminished, the urine is seldom in such cases altogether suppressed.

---

[1] "Cyclopædia of Practical Medicine."

The frequency of the suppression of urine, in connection with perforation of the ilium, requires to be further noticed, from its having induced M. Cazeneuve,[k] and subsequently M. Auguste Judas,[l] to publish some papers on the subject, in which they attach great importance to it, as a diagnostic sign of peritonitis resulting from this lesion. From the cases related by these writers, it is evident that the suppression of urine was for the most part different in its character from that witnessed in cholera, and may be considered to have been the result, in some measure, of irritation of the urinary organs, producing strangury; in this respect, agreeing with Dr. Stokes' observations on the same subject. M. Cazeneuve, however, seems disposed to attach great importance "to the existence of other abnormal secretions," in causing the urine to be suppressed in these cases; and as this idea has been very generally entertained, respecting the same symptoms in cholera, it may be useful to notice the arguments employed in support of this opinion. "It is known (observes M. Cazeneuve) that functions of the same nature are so connected together, that if one increases, the other diminishes, or disappears. Now, in all the cases I have observed, or read of, there exists a very profuse purging, the patient having five, six, ten or twenty stools. It is not, therefore, surprising that the urinary secretion is diminished, or suspended. It is so in all cases of abundant flux from the alimentary canal, in cholera, and in chronic diarrhœa..... It is necessary to join to this cause the existence of an abnormal secretion in the peritoneum, in which pus and serosity accumulate. No one can deny the relation which subsists between this abnormal secretion, and that which takes place from the kidneys and intestinal canal; for it is not unfrequently observed, that ascites disappears by the urine and the stools. We every day take advantage of these facts, these indications; and the results often confirm the utility of these precepts." Admitting, for the sake of argument, the correctness of M. Cazeneuve's remarks respecting the diminution, or suppression of the watery parts of the urine, the question will still recur, What becomes of the urea? M. Cazeneuve fails to inform us directly, but apparently wishes it to be inferred, that the urea is disposed of by the complementary discharge. This supposition is opposed by the results of the analysis of the alvine evacuations in cholera, and is contrary to the evidence furnished by those cases in which suppression of urine occurs, independent of any abnormal, or greatly increased discharge from other organs, as in some cases (at least) of perforating ulcer of the stomach. But it is not only in these last cases that suppression of urine, independent of disease of the kidneys, may occur without any complementary discharge from other parts of the system. For the same symptom has been observed in connection with certain cases of obstruction of the small intestines, and also as generally accompanying intersusception in children.

---

[k] "Gaz. Med. de Paris," 1837, No. 52; et 1838, No. 1.

[l] "Gaz. Med. de Paris," 1838, No. 46.

In some remarks which have been published on this subject by Dr. Barlow,[m] it is interesting to notice how differently the same symptom may be interpreted, so as to be made apparently to support an opposite theory. Whilst, on the one hand, M. Cazeneuve has endeavoured to prove that suppression of urine is a valuable diagnostic sign of perforation of the ilium, and as such is always accompanied by a discharge from some other organ, Dr. Barlow considers that it is diagnostic of that form of constipation which results from obstruction of the bowels situated high up. That when it occurs in connection with constipation, it indicates that the cause of the obstruction to the bowel is high; if the obstruction is low down, then the urine is not suppressed in these cases of obstinate constipation. "If a sufficient quantity of water (Dr. Barlow observes) cannot be received into the small intestines, or if the circuit through the portal system into the vena cava ascendens, or thence through the lungs and heart into the systemic circulation, be obstructed, or if there be extensive disorganization of the kidneys, the due secretion of urine cannot be effected." Having enunciated this proposition, Dr. Barlow endeavours to shew that the suppression of urine in cholera is due to the great quantity of fluid discharged by stool, in consequence of which there is "a want of water in the blood to carry out in solution those substances which should be eliminated by the liver and the kidneys, and which are consequently retained in the system, and there act as poisons." In the case related by Dr. Barlow, on which this opinion is chiefly founded, it appears that the patient, a boy aged twelve years, died in consequence of constriction of the jejunum by a cellular cord. No urine was passed for five days, and on a catheter being introduced, "one ounce, pale and straw-coloured, and free from albumen, was obtained." This statement is strongly opposed to the idea that the suppression could be due simply to the absence of a sufficient quantity of water to dissolve out the urinary compounds from the blood. For if such were the case, we should expect that the little urine secreted would have been loaded with the usual solid constituents, as occurs in other cases, where, from a deficiency of the watery parts, the quantity of urine is diminished, but the solid constituents are proportionately increased. The fact of the urine being pale and straw-coloured, shews that the suppression must have been due to some cause, deeper seated than the mere absence of a sufficient quantity of water from the blood; and it can only be satisfactorily explained by assuming that the formation of the peculiar constituents of the urine in the blood, is for a time arrested. The case, related by Dr. Barlow, appears, indeed, to confirm the opinion I have advanced respecting the suppression of urine in cholera; by supplying satisfactory evidence that prolonged suppression of urine, similar in character to that which accompanies cholera, may occur independent of any complementary discharge from other organs, as a direct consequence of shock to the sympathetic nervous system; that shock being communicated to the stomach, either direct, or through the upper portion of the intestinal canal.

---

[m] "Guy's Hospital Reports," 1844.

In all the cases which have been referred to, in which the urine has been suppressed, without causing any head symptoms, the primary seat of the disorder can be clearly traced to the stomach or intestinal canal; and there can consequently be little hesitation in ascribing the symptoms to an impression on the abdominal portion of the sympathetic nervous system. For although the office of the sympathetic nerve has not been clearly ascertained, and it can only be supposed as yet, from its distribution, that this part of the nervous system is, in a special manner, connected with the organs designed for the nutrition of the body, it is not unreasonable to infer, even in the present state of our knowledge on the subject, that it is the medium for communicating that influence which regulates the functions subservient to nutrition; and that the urine is suppressed in consequence of some great disturbance affecting the central organ for nutrition.

The extent to which the formation of urea in the blood is affected, after its elimination by the kidneys is arrested.

The fact that prolonged suppression of urine may occur in cholera, without giving rise to coma, has been confirmed by the testimony of medical men in all parts of the world in which the disease has prevailed, and has justly been looked upon as one of its most characteristic symptoms. Previous to the appearance of cholera, it was very generally believed that the secretion from the kidneys could not be arrested for any considerable period of time, without being followed by certain symptoms, ascribable to the blood becoming poisoned by the retention in it of the essential principle of the urine, urea. The duration of life, under these circumstances, in a case observed by Dr. Abercrombie,[n] did not exceed five days; which accords with Dr. Prout's experience on the same subject, who states that "when the suppression of urine is complete, coma almost invariably becomes established before the fifth day."[o] MM. Prévost and Dumas' experiments,[p] confirmed by numerous observers, of removing the kidneys from animals, tend to establish the correctness of these observations, by shewing that after the extirpation of the kidneys, death in like manner usually occurs by the fifth day.

---

[n] "Edinb. Med. and Surg. Journ." vol. xvii. p. 216.

[o] "On Stomach and Renal Diseases," 5th edit., 1848; p. 425.

[p] "Annales de Chimie," tom. xxiii.

The most interesting and important point, in connection with the suppression of urine in cholera, is consequently to determine what becomes of the urea. There have been, as might be expected, many and very opposite opinions on this subject; among which it is necessary to notice, first, the old and frequently rejected theory, that the urea is accumulated in the system, and acts as a poison; as I find that this opinion is still maintained by some writers on cholera, and among others by Dr. Barlow,[a] to whose paper I had occasion to refer, in speaking of the condition of the urinary organs. This theory is not only opposed to our experience of many cases, in which very prolonged suppression of urine has not been followed by any evidence of uræmic poisoning, but it is refuted by the analyses of the blood in cholera, which prove that no such accumulation of urea occurs in the collapsed stage of the disease. The blood of cholera patients has been subjected to frequent analyses; and any doubts which may have been previously entertained respecting the amount of urea in it, under these circumstances, may be considered as satisfactorily settled by the researches of Dr. Garrod. This observer has ascertained that during the stage of intense collapse, the quantity of urea in the blood is but small, that it increases during reaction, and is in excess when consecutive febrile symptoms occur. The following remarks which he makes on the results of his analyses, completely accord with the views I entertain respecting the nature of the suppression of urine in these cases. "I should imagine (Dr. Garrod observes) that in intense and sudden collapse, not only is the function of the urinary excreting organ diminished or suppressed, but also the vital metamorphosis, and therefore the formation of urea, are likewise nearly suspended. This would account for the small amount usually found in collapse; and probably the quantity varies inversely with the intensity of this state; but when partial reaction ensues, and the vital changes take place with greater activity, should the functions of the kidneys not be at the same time restored, urea must accumulate in the blood, and the amount must depend on the degree of the reaction (febrile or not) and the extent of the suppression of the urinary secretion. This view is certainly supported by the results which have been as yet obtained, not only recently by ourselves, but also in the former epidemic, by Dr. O'Shaughnessy and others."[r]

---

[a] "Guy's Hospital Reports," 1848.

This absence of urea from the system has been very oppositely explained by different writers on the subject; by one party it has been assumed, that the urea is disposed of by a vicarious discharge; by the other, that the formation of urea is for a time altogether arrested. The first of these opinions is the one which has been the most commonly entertained, and

[r] "London Journal of Medicine," 1849, p. 435.

is still supported by some of the most distinguished members of the profession; who allege that the alimentary mucous membrane is capable of performing, under these circumstances, vicariously, the office of the kidneys. Without entering into any general discussion on the subject of vicarious secretion, it it will be desirable to consider what evidence can be adduced in favour of this hypothesis, as far as the kidneys themselves are concerned; the term "vicarious secretion," being used in a restricted sense, to express that the peculiar function of one organ is performed by another; and not applying to such cases as the so-called vicarious menstruation, in which the discharge is sanguineous, and may consequently, under certain abnormal circumstances, be the result, not of a process of secretion, but of exudation.

The cases in which, independent of cholera, the urine has been alleged to be discharged vicariously, are chiefly, if not altogether, dependent on hysteria; and they form a small and extremely doubtful group of cases, known under the name of "Hysterical Ischuria." I necessarily exclude from consideration some very remarkable cases, in which suppression of urine has existed from birth; for it appears that in these cases, the ureters, in consequence of congenital malformation, in place of opening into the bladder, communicate with some portion of the alimentary canal; which is the normal condition of these parts in the monotremata.

In common with almost every other symptom of disease, suppression of urine has been sometimes observed in connection with hysteria; and in some of these cases the suppression has been said to continue for months, and even years, without any bad consequences to the health of the patient; owing, it has been supposed, to the urine being got rid of by a vicarious discharge. The most frequent of these alleged abnormal discharges, is that which takes place from the stomach, by vomiting at periodic intervals, and consists of a fluid more or less closely resembling that ordinarily discharged from the bladder. There can be no doubt that in the majority of these cases the suppression of urine, as well as its vicarious secretion, has been feigned; and some curious examples have been recorded of hysterical females, who have succeeded in making their attendants believe that the urine, which should have been passed by the natural channel, has been habitually discharged from the stomach by vomiting, or disposed of by some other outlet, as the ears, the umbilicus, etc. In a paper on this subject, by Mr. (now Dr.) Laycock,[a] there are collected together the histories of some remarkable examples, observed by himself and others, of this counterfeit vicarious discharge of urine; in some of which the patients were detected in having swallowed the urine, which was subsequently rejected from the stomach by vomiting. But although deception has been undoubtedly practised in most of these cases, there is reason to believe that in some of them a real suppression of urine did occasionally occur, but without giving rise to any vicarious discharge. In reference to this point, Dr. Laycock remarks, "Hysterical Ischuria, in a mild form, is of no uncommon occurrence, and doubtless is frequently overlooked by or concealed from the practitioner. A young female will suffer for twenty-four or thirty-six hours, a suppression of urine. If the hand be placed upon the hypogastrium no distension of the bladder will be observed. She is perhaps alarmed at the occurrence of this long interval, the pulse will be found much quickened, but nothing otherwise remarkable will be noticed. At last the patient voids a few tablespoonfuls of urine, and nothing more is heard of its suppression."

---

[a] "Edinb. Med. and Surg. Journ.," 1838.

In admitting that a temporary suppression of urine is one of the symptoms by which hysteria may sometimes manifest itself, I must at the same time reject the idea of the urine ever being disposed of in these cases by vicarious secretion. The tendency in hysterical patients to simulate diseases, especially of the urinary organs, should induce us to doubt their existence, unless the evidence in their favour is undeniable. This, however, is far from being the case in hysterical ischuria: for where the patient has been carefully watched for some time, imposture has been detected in so many instances, as to lead almost necessarily to the inference, that it has been practised in all. I am aware that many distinguished members of the profession differ from me on this point,[t] but I do not think any evidence has been brought forward by them, in these cases, sufficient to warrant us in admitting a theory, which involves an assumption so unnatural as that of vicarious secretion.

---

[t] Dr. Wood, in his remarks on Hysteria, observes that "the urine

It remains, therefore, to be considered, whether, if the vicarious secretion of urine is restricted to cholera, there is sufficient evidence to prove, that the essential constituent of the urine is present in such quantity in the flux from the stomach and bowels, as to favour the supposition, that it results from vicarious secretion. The recent analyses of the evacuations in cholera, by Dr. Parkes,[u] appear to have satisfactorily determined this point in the negative; in showing that the amount of urea, in such cases, is no more than what we might expect to find in a discharge, which is derived from the liquor sanguinis, by a process which differs in some respects from that which effects the ordinary secretion of the part, but not so as to justify the inference, that the peculiar function of one organ can be transferred to another,

---

is sometimes so copious as to imitate diabetes, and in some rare cases has been almost or entirely suppressed. In the latter condition, the constitution appears occasionally to protect itself by a secretion of the urinary principles through some other outlet; and it is impossible to resist the evidence in favour of those cases in which a liquid, having the odour of urine, is asserted to have been thrown out by the umbilicus, the external meatus, the stomach, etc.; though it must be confessed that great allowance should be made for the insane propensity to deception, evinced by some females under these circumstances."—" A Treatise on the Practice of Medicine," 4th edit., vol. ii. p. 838.

[u] " London Journal of Medicine," 1849.

or that either the stomach or the intestines can be even temporarily converted into kidneys. The evidence, indeed, on this point has been considered so conclusive, that some of the advocates of vicarious secretion, unwilling to relinquish the idea of the urea being secreted as such by the alimentary mucous membrane, have endeavoured to account for its absence in the cholera evacuations, by suggesting that it may be thrown out into the bowels, and there decomposed into salts of ammonia.[1] This supposition is based on some experiments by MM. Bernard and Barreswil, which have been thought to prove that, after extirpation of the kidneys, coma is averted so long as the intestinal evacuations continue to contain salts of ammonia.

Independent, however, of the unsatisfactory evidence derived from experiments, which cannot be performed without causing general disturbance in the system, there is nothing to prove that the salts of ammonia in these cases result from the decomposition of urea. On the contrary, we have an equal, if not a greater right to assume, that owing to the general disturbance of the circulation, consequent on the shock caused by the operation, the changes which usually take place in the system are not completed; and that the absence of urea may be due to a deficiency of organic power, from the failure of the circulation in that part of the system, in which the urea is principally, if not altogether, formed. This would allow of the supposition, that the force which is insufficient to effect the formation of urea, may be sufficient to form carbonate or other salts of ammonia; or it might even be supposed, that the alkaline salts, which are said to be formed in these cases in place of urea, may result from a chemical rather than a vital change taking place in the system. The presence of the alkaline salts in the bowels, under these circumstances, would consequently be supposed to indicate, that the system is unable to form the organic and higher compounds of oxygen.

---

[1] "A Treatise on the Practice of Medicine," 4th edit. vol. ii. p. 599.

But, assuming that the results of MM. Bernard and Barreswil's experiments are correct, I should be more inclined to regard the presence of the alkaline salts in the alvine evacuations, as an indication of the perverted secretory function of the alimentary mucous membrane, consequent on shock, and that these salts are derived from the ordinary constituents of the blood, by a process similar to that which produces the serous flux in cholera.

If the preceding arguments have been sufficient to exclude the idea of vicarious secretion of urine occurring in cholera, it must necessarily be assumed, that the absence of urea is the result of its formation in the blood being interrupted. The extent of this interruption during collapse, may be judged of from the results of the analyses of the blood of cholera patients by Dr. Garrod and others; but it also admits of being still further illustrated by the examination of the first urine passed after the attack, especially in those cases which are not accompanied with consecutive fever. The urine, which is passed under these circumstances, instead of being rich in urea, as is generally observed to be the case in that passed after (partial) suppression of urine from other causes, has been found to contain less than the normal amount, and in some instances the urea has been altogether wanting. Dr. Begbie[y] states, as the result of numerous experiments on the first urine passed after an attack of cholera, that "the average density was considerably lower than that of healthy urine"; and with reference to the amount of urea contained in it under these circumstances, that of eighteen specimens of cholera urine, specially examined to determine this point, that in all of them the urea was found to be deficient in quantity. Dr. Begbie deduces from his observations—1st, "That the urine, besides being affected as to quantity is materially altered in quality"; and 2nd, "That this alteration consists in the presence of a very small quantity, or in the entire absence of urea, and in the presence of albumen and biliary colouring matter." Still stronger evidence is furnished on this point by Dr. Parkes, in the following case:—"A boy who had presented all the algide symptoms (of cholera) in moderate intensity, passed no urine for fifty-three hours, after which time he passed about ten ounces of clear yellow urine, free from sediment on standing, and in which no urea was detected."[1]

---

[y] "Edinb. Month. Journal of Med. Science," Nov. 1849, pp. 1207—1213.

It is a remarkable but well observed fact, that the urine which is sometimes passed during the continuance of the collapse from cholera, is also deficient in urea, although the quantity of urine passed under these circumstances may be inconsiderable. This has been occasionally noticed in all the successive epidemics of cholera, and it has never been satisfactorily accounted for. Scot, Orton, and most of the earlier writers on the disease, who have mentioned it, pass it over almost without comment; and we may judge of the difficulties these writers must have experienced in attempting to explain the phenomenon, from Dr. James Johnson's remarks, which may be said to represent the state of contemporary medical opinions on this subject. "When this secretion (Dr. Johnson observes) is not suspended during an attack of cholera, the urine is almost always limpid and clear, though in very small quantity—a curious phenomenon, considering the probable state of the blood under such circumstances; for we may be permitted to infer, from all the symptoms, that the blood is not only deprived of much of its serous or aqueous parts, by the profuse discharges which usually take place, but that the elements of all, or most of the other natural secretions, are retained in it. We might, therefore, naturally have expected that, if urine were secreted at all, it would possess some striking deviation from its natural appearance."[a] This deficiency of urea in the urine passed by cholera patients, has been noticed by many subsequent writers, among whom I may refer to Dr. G. Owen Rees, who states "that when it (the urine) can be collected in any quantity, its specific gravity is generally below the natural standard, and it has only a feeble acid reaction. Albumen is very often to be found in it. The urea is below the standard of health."[b]

---

[1] "London Journal of Medicine," 1849, p. 138.

The deficiency of urea, which is so remarkable a phenomenon in connection with the suppression of urine in cholera, occurs also in cases of spontaneous perforation of the stomach, and also in some cases of spontaneous perforation of the small intestines.[c]

This is very different from what is commonly witnessed in cases of suppression of urine from other causes, in which the first urine passed after the suppression is generally observed to be loaded with organic matter, which seems to be ready to find an exit from the system, as soon as the usual channel for its discharge is open for it to pass. In cholera, no such accumulation occurs during collapse; but, on the contrary, a certain time seems to be required after the collapse has passed off, for the circulation to be restored, so as to enable the system to resume those functions which constitute organic life.

[a] "Diseases of Tropical Climates," 5th edit. 1836, p. 276.

[b] "Cyclop. of Anat. and Phys.," art. Urine.

[c] "Gazette Med. de Paris." 1838, No. 46, p. 721.

The extent to which the nervous power influencing the circulation of the blood, and through it the secretion of urine, is affected by cholera, is not only so great as to interrupt the formation of urea during the continuance of collapse, and also after this condition has passed off; but, even in individuals who have escaped being attacked by the prevailing epidemic, the energy of the system is observed to be not only temporarily affected, but to continue in many cases defective, and insufficient for the maintenance of a healthy state of the functions. One of the consequences of this condition appears to be the substitution of oxalic acid for urea. In reference to this point, Dr. Prout observes, "Every one is acquainted with the familiar fact, that the most frequent and striking morbid appearances presented by the urine from slight causes, (such as a cold, indigestion, etc.), are the common lateritious sediments. Now, the first circumstance that attracted my notice after the prevalence of the Asiatic cholera, was the disappearance of these sediments from the urine. The absence of these sediments was at first considered to be accidental, but when day after day the same occurrence took place, I was induced to inquire attentively into the circumstances, with the view, if possible, of ascertaining the reason. On closer inspection, it was found that the urine of every individual examined, whether in apparent health or otherwise, not only presented the same absence of sediment, but also assumed that peculiar appearance, which I have been accustomed to consider as characteristic of the presence of oxalic acid. As I had always previously found the oxalic acid diathesis of unusual occurrence in London, I was much struck with the phenomenon; and on reflection it occurred to me, that it might be referrible to the same unknown cause which was then producing cholera." [d] It would be difficult to appreciate too highly these observations by Dr. Prout, which are very suggestive of the causes which may lead to the non-formation of urea in the blood, and the conditions which may be required to effect the substitution of oxalic acid for urea. During the last epidemic of cholera in this country, I had the opportunity of testing the correctness of Dr. Prout's statements; and I can vouch for the general accuracy of his remarks on this subject.

As urea is one of the constituents of the urine, which has been experimentally proved to be formed in the blood independent of the kidneys, which appear, however, to possess the exclusive power of withdrawing it from the blood, in which it exists ready formed for elimination, it becomes an important point to determine, not merely whether its elements are derived, as generally supposed, "from the unassimilated elements of nitrogenized food, circulating with the blood," and "from the disintegration of the azotized animal tissues;" [e] but also whether its formation is not restricted to certain parts of the system, and does not take place indifferently in all parts through which the blood circulates.

---

[d] "On Stomach and Renal Diseases," 5th edit. 1848, p. 20.

[e] "Handbook of Physiology," by Kirkes and Paget, 1848, p. 341.

The amount of urea formed within a given time appears to depend on the waste to which the muscular tissues are subjected; and this waste is connected with (dependent on?) the state of the circulation of blood through them. It is not, therefore, unreasonable to suppose that the muscular tissue is specially connected with the development of urea, not simply as the source from which its elements are derived, but as the structure in which its formation is accomplished. There are many arguments which might be adduced in support of this opinion, which appears, indeed, to be perfectly conformable to our every-day experience of muscular change. When the blood accumulates, to any great extent, in the internal organs, especially of the abdomen, the force of the circulation through the extremities and towards the surface of the trunk is diminished; and consequently the functions usually performed in these parts of the system may be inferred to be also proportionately diminished, if not arrested. Whilst, on the other hand, if the general force of the circulation is increased, the functions of all parts of the body are also increased in proportion to the amount of blood supplied to them.

The well-known effects of much exertion in walking, etc., in increasing the amount of urea, may, therefore, be caused by the increased force of the circulation, and the consequent increased supply of blood furnished to the muscles employed on the occasion, producing a corresponding waste of their tissue. This waste is restored during that sound and refreshing sleep which healthful exercise always (secondarily) induces; and the feeling of fatigue is relieved when the tissues have been repaired. But if the exercise has been excessive, the feeling of fatigue caused by the waste of the tissues will be excessive also, and will prevent the occurrence of sleep. The demand for sleep in these cases might be inferred to be greater, from the condition of the urine, which shews a greatly increased formation of urea in the system; and as the absence of sleep delays the repair of the body, the formation of urea continues to keep pace with the increased circulation, so long as the force of the latter is maintained. This contributes to produce that feverish condition of the system, which subsequently accompanies the continued and increased formation of urea, and which is prejudicial in proportion as sleep is postponed.

But other phenomena than those which have been mentioned result from over fatigue; the most important of which is the diminution or arrest of the digestive function of the stomach. The disrelish for food which individuals suffering from over-exertion experience, and the inability of the stomach under these circumstances to digest it, depend chiefly on the blood not being sufficiently determined to the stomach after the food has been introduced into it; and, therefore, the circulation through the muscular (and necessarily also the other) tissues of the body, continues to be maintained, and the consequent waste produces a corresponding amount of urea. The beneficial effects which result from stimulants taken with the food on these occasions, depend on an increased supply of blood being sent to the stomach; and which afflux of blood may in health be always inferred to occur, from the superficial coldness which follows a meal. Whilst the well-known effects of tea in relieving the feeling of fatigue, and in overcoming the feverish state which follows excess in eating, appears to depend in a great measure on the property which the tea possesses of moderating, and at the same time of equalising, the circulation of the blood. There is, also, some other important property in tea, which has the power both of arresting the waste, and also of preventing the renewal, of the tissues; but it is not apparent in what way this effect is produced; it is, however, probable that this is the primary effect of tea, and that the equalisation of the circulation results from it.

The necessity in the system for food is expressed by the stomach, and the digestive function of the stomach appears to possess a greater influence over the circulation of the blood in health, than that possessed by any other function of the body. But when the claims of the stomach have not been attended to, and the rest which the body requires to effect the necessary repairs, have not been allowed, the waste of the tissues is apt to increase rapidly, and the stomach then becomes unable to satisfy the wants of the system; for nature abhors excess, and never willingly favours one part of the system at the expense of another. This is constantly witnessed in fevers and other diseases, where the circulation is greatly increased, and there is a corresponding waste of the tissues, whilst the digestive powers of the stomach are proportionably diminished. The increased formation of urea which occurs under these circumstances, contrasts strongly with its suspended formation in cholera and other diseases, where the blood being determined to the viscera of the abdomen, the general circulation fails.

The preceding illustrations may be said to afford good presumptive evidence in favour of the muscular tissue being the principal, if not the only source of the urea: and this opinion is supported by Dr. Marchand's experiments,[d] which have been repeated and confirmed by other observers; and which shew that in animals deprived altogether of food, the formation of urea still goes on in the system. From this it is evident that whatever other sources there may be for the urea excreted, it can be formed in the system independent of the food. The feverish condition which accompanies prolonged fasting, and which not only accounts for the subsequent inability of the stomach to digest food (for if the abstinence has been very prolonged, food may be taken too late to save life), but also for the morbid appearances presented by the stomach after death from starvation, is consistent with the continued excretion of urea, and appears to confirm the explanation I have given, respecting its continued and increased formation in this and corresponding states of the system.

---

[d] "Muller's Archiv.," 1839, p. 90.

For further confirmation of this opinion I may refer, in conclusion, to some cases of an opposite description, where, owing to a failure of the systemic circulation, similar to what is witnessed in cholera, and perforating ulcer of the stomach, the formation of urea is also observed to fail, either with or without suppression of the secretion from the kidneys. The aqueous character of the urine passed during the cold stage of ague shews that the secretory function of the kidneys continues after the formation of urea in the system is interrupted; owing, as we may suppose, to the primary seat of the disorder in ague, being referrible to some other organ less important than the stomach is to the rest of the system. That this is to a certain extent the case. may be inferred from the action of certain poisons, which cause a failure of the systemic circulation, and in which the direct effects of the poison are manifested on some portion of the alimentary mucous membrane, leading to a great reduction in the formation of urea, and sometimes complete suppression of urine. The well-known diuretic effect of foxglove seems to be independent of any increased formation of urea in the system, for the urine which is passed under these circumstances is very watery, and usually deficient in urea. This corresponds with the depression of the systemic circulation which foxglove always causes; and it has been confirmed by an interesting fact which has been observed in connection with an overdose of this medicine; viz., prolonged suppression of urine, accompanied by symptoms closely resembling those witnessed in the collapse of cholera.[e] This subject may be still further illustrated by referring to the poisonous effects of lead on the system, which is also apt to cause a failure of the systemic circulation,[f]

---

[e] The following case of "Suppression of urine resulting from an excessive dose of Foxglove," was observed by the late Dr. William Henry, physician to the Manchester Infirmary. The patient was a woman, aged sixty, suffering from Ascites, and swallowed about ten ounces of an infusion of Foxglove, at the recommendation of a friend. Soon afterwards, Dr. Henry states, "she began to be sick, and discharged part of the contents of the stomach. Enough, however, was retained to excite violent retching and vomiting throughout the whole of that and the following day, during which everything that was taken was instantly rejected. In the intervals of the sickness she was excessively faint, and her skin was covered with a cold sweat; the tongue and lips swelled, and there was a constant flow of viscid saliva from the mouth. Very little urine was voided on Sunday, and on the two following days the action of the kidneys was entirely suspended. When I saw her on Tuesday (two days after taking the Foxglove), the sickness had somewhat abated, though it was still extremely distressing. The tongue was covered with a white fur; the ptyalism continued, though in a less degree, and the breath was fetid. The pulse was low, irregular (not exceeding forty), and after every third or fourth pulsation an intermission occurred for some seconds. She complained, also, of general pains in the limbs, and cramps in the legs."—"Edinb. Med. and Surg. Journal," 1811, pp. 148, 149.

[f] Contrairement à l'illustre medecin de Vienne (Stoll), nous avons constaté un pouls petit, grêle, mou, facile à deprimer, chez les ouvriers exposés à respirer et à avaler une grande quantité de plomb (ouvriers des fabriques de céruse, de minium, etc. etc.).

"Dans quelques cas rares on rencontre, en même temps que les with consequent diminution in the formation of urea, and occasionally even suppression of urine. This last symptom, it must be admitted, is rare; and it might be inferred to be so, supposing M. Tanquerel to be correct in ascribing Colica Pictonum to neuralgia of the abdominal sympathetic nerve. But when lead has had the effect of causing, as it sometimes appears to do, partial paralysis of the sympathetic nerve, distributed to the alimentary canal, there seems to be a consequent arrest of the blood in this part of the system, similar to what occurs in cholera, and suppression of urine is one of its results; provided the sympathetic nerves distributed to the kidneys do not share in the paralysis.

2. *The altered condition of the blood.*

The transformation of the blood in the veins of cholera patients into a thick, black, and sometimes uncoagulable mass, which, after a time, is found to supersede the red blood in the arteries, exhibits a change rarely witnessed in any other diseases; and

---

alterations précédentes de la circulation artérielle, un ralentissement marqué des pulsations; le pouls bat 40, 45, 50, 55 fois par minute, tandis que l'ouvrier, avant de manier le plomb, avait un pouls de 60 à 70 pulsations. Cette diminution des battements artériels s'observe également chez les individus qui font usage à l'intérieur de médicaments composés de préparations saturnines."—"Traité des Maladies de Plomb ou Saturnines," par L. Tanquerel des Planches, Paris, 1839, tom. i. p. 18; voyez aussi tom. i. pp. 227, 228.

which has induced many observers to ascribe the primary cause of cholera to some morbid affection of the blood—the result of a specific blood-poison. There is, however, no proof that the blood, taken from a patient at any period of the disease, is capable of communicating cholera. M. Foy,[h] one of the medical men sent by the French Government to investigate the disease at Warsaw, in 1831, states that he inoculated himself in two places with the blood flowing from the vein of a cholera patient and suffered no ill effects from it. The innocuity of the blood of cholera patients was still further established by M. Rayer,[i] who performed numerous experiments on animals, and found that inoculation with cholera blood failed to give rise to any symptoms analogous to cholera, or to produce any change corresponding with what is usually met with after death from this disease.

The consistence of the blood is increased in consequence of the abstraction of a large quantity of the water, with some of the solid constituents of the liquor sanguinis, leaving an excess of the red globules, at the same time that the other solid constituents of the blood, although absolutely diminished, maintain a relative excess. This increase in the solid constituents of the blood causes a corresponding increase in its specific gravity, which in one of the cases recorded by Dr. Garrod,[k] was observed to be 1081·8.

---

[h] "Lancette Française," Juillet 2, 1831; aussi "Histoire Médicale du Choléra-Morbus de Paris," par F. Foy, Paris, 1832, p. 44.

[i] "Lancette Française," 1832, p. 320.

The diminution in the water of the blood is the result chiefly of the flux from the surface of the alimentary canal; some of the diminution is also to be accounted for by the exudation from the skin, but this is of far less consequence than that which is lost in the intestinal discharges. As the necessary effect of this loss of the watery constituents is to render the blood less fluid, the consistence of the blood may be roughly assumed to be inversely in proportion to the amount of the flux. The opinion entertained by many writers that the consistence of the blood is in direct relation to the intensity of the collapse, can only, therefore, be correct, so far as the collapse is dependent on the discharges; and as I have endeavoured, in a subsequent passage, to prove that there is no necessary connection between them, I would merely observe here, that in the most intense form of collapse, in which death supervenes quickly, with little or no flux from the alimentary canal, the blood usually retains its fluidity.

The relative proportion of red globules to the mass of blood in cholera, has been observed by Dr. Garrod to range as high as 171 in 1000; 140 in 1000 being considered a large amount consistent with health.

Respecting the fibrine, Dr. Garrod remarks, "it is probable that in cholera this element of the blood undergoes changes of quality rather than of quantity; and that as long as it can be ascertained correctly, analyses do not indicate any marked deficiency; after a time, however, it can be no longer collected." In one of the cases recorded by Dr. Garrod (Case 5), the quantity of fibrine was higher than usual, being 2.61 in 1000 parts of blood, and yet the blood did not coagulate, the fibrine having undergone some change, which was physically shewn only in its being "less consistent than natural." In other cases, the proportion of fibrine has been found to be less than 1 in 1000 parts of blood. So that whilst probably there is no great alteration in the relative amount of fibrine in cholera blood, there is a great absolute diminution compared with the red globules.

---

[k] "London Journal of Medicine," 1849.

The changes to be noted in the composition of the serum, are a relative increase in the albumen, and in some of the salts, owing principally to the flux from the stomach and bowels not removing them in the same proportion as it does the water and some of the other saline ingredients. The exact nature of the change affected in the saline constituents of the serum has not been satisfactorily ascertained, and further researches are required to enable us to state the character and amount of the salts abstracted, and of those retained in the blood. One of the most important points connected with the salts of the serum, refers to the amount of urea present in the blood of cholera patients during collapse, and this has been settled by the researches of Dr. Garrod.

It is now well known, that as urea is formed in the blood, and thence eliminated by the kidneys, its presence in the blood, in small quantities, is consistent with health; but as the amount of urea that can usually be detected in healthy blood is exceedingly small, it has been inferred that the kidneys withdraw it very soon after its formation. In cholera, it was for a long time supposed, that urea continued to be formed, and that owing to the functions of the kidneys being arrested, it accumulated and poisoned the blood. The analyses of the blood in cholera have proved, however, that the amount of urea present is generally very small during collapse. Dr. O'Shaughnessy[1] found that in the serum of the blood "taken from a patient labouring under severe blue cholera," the proportion of urea was only 0.40 in 1000 parts; and from the more recent researches of Dr. Garrod, already referred to, it may be inferred, that the formation of urea is almost, if not altogether arrested during collapse.

The changes which have been referred to in the composition of the blood, are sufficient to account for the alteration effected in some of its properties, as increase of consistence, and consequently of specific gravity; but there are other changes to which the blood appears to be liable in cholera, affecting (*a*) its chemical reaction, (*b*) its colour, and

---

[1] "On the Chemical Pathology of Malignant Cholera," 1832, p. 61.

(*c*) its coagulation; each of which will require to be more carefully considered, from having been the cause of much error respecting the nature of the disease, and from having each of them been differently interpreted by different observers.

(*a*).—*On the chemical reaction of the blood in cholera.*

One of the most difficult subjects to be considered in connection with the blood in cholera, is the diminution in its alkalinity, which appears to be proportioned to the amount of the flux from the alimentary canal and the consequent loss of fluidity in the blood. In many cases where the consistence of the blood has been much increased, its reaction has been observed to be neutral, and sometimes even distinctly acid. It is uncertain at present, on what this altered chemical constitution of the blood immediately depends; whether it is owing to the alkaline salts being removed in greater proportion than the other saline constituents, or whether, as many have supposed, it is due to the presence of a free acid in the blood. Dr. O'Shaughnessy seems inclined to ascribe it to deficiency of carbonate of soda, which he observed to be diminished, or altogether wanting, in some specimens of cholera blood which he examined. Dr. Garrod, on the contrary, states "that it is not due to the loss of salts, but to the impeded excretion of organic acids, which are constantly being formed in the system." I cannot say that I think either of these opinions consistent with the change which takes place in this instance. For Dr. Garrod's statement that the loss of alkalinity in the blood is due to the accumulation of organic acids, "which are constantly being formed in the system," is opposed by many other phenomena in cholera, which shew that oxidation, so far from being increased, is greatly diminished, if not altogether arrested in most parts of the system during collapse. Whilst the supposition that it is due to the abstraction of carbonate of soda, is a very doubtful explanation of the change; for the absence of carbonate of soda, supposing it to exist in such quantities in healthy blood, as to be the cause of its alkaline reaction, may, in cholera, be ascribed with greater probability to the non-formation of carbonic acid, rather than to the abstraction of this alkaline salt. I should be more inclined to adopt Liebig's view respecting the cause of the alkalinity of healthy blood, and to regard the absence of the tribasic phosphate of soda as the principal cause of the blood in cholera being less alkaline than in health.[m] If it should be subsequently proved that this alkaline phosphate is the principal cause of the blood in health possessing an alkaline reaction, it may be inferred that the loss of this property in cholera blood depends on this salt being withdrawn from the blood in greater proportion than some of the other saline constituents. The process by which this change is effected, cannot be ascribed to what is commonly called exudation, which precludes the idea of any one or more of the saline constituents of the liquor sanguinis being abstracted in preference to the rest. The fact of such selection having occurred must be received as evidence in favour of the supposition that the loss which is sustained by the blood in cholera is the result of a process of secretion. Consequently, if the flux accounts for the change effected in the composition of the blood by the removal of certain of its constituents, the cause of the flux must be due to increased secretion from the mucous membrane of the alimentary canal. This perfectly coincides with the theory which ascribes cholera to paralysis of the central portion of the sympathetic nervous system, excited through the medium of the stomach; but it does not admit of being more fully considered, until the characters of the flux have been described.

---

[m] "Researches on the Chemistry of Food," by Justus Liebig translated by Gregory, 1847, p. 112.

(*b*).—*On the colour of the blood in cholera.*

The change effected in the colour of the blood by cholera, has been ascribed to various causes by different writers, in consequence partly of the conflicting opinions which have been entertained concerning the nature of the disease, but chiefly owing to the uncertainty which has prevailed respecting the cause of the change which takes place in the colour of the blood in health.

It has been supposed by Annesley,[a] and others, that the dark colour of cholera blood is due to excess of carbon, and that the colour becomes darker in proportion as the accumulation of carbonaceous matter in the blood is increased. This opinion was at one time very generally adopted, and has even been advocated by some recent writers, who seem to think that the carbonic acid given off in respiration, is the result of the union of free oxygen with free carbon, either in the lungs, or in the course of the circulation. There is, however, no evidence to induce us to suppose that under any circumstances, or in any form, is free carbon developed and accumulated in the blood; for the changes which ordinarily take place in the blood in consequence of respiration, in giving rise to the formation of carbonic acid, do not indicate that oxygen meets, in the course of the circulation, with carbon in a free state; on the contrary, it is probable that none of the elements exist as such in the blood, but always in combination; and that oxygen itself probably forms, as it enters the blood, a chemical union with some of the constituents of the blood, and is never present as free oxygen.

The supposition that the dark colour of cholera blood is owing to a deficiency in the supply of oxygen by the lungs,[b] has always been a favourite way of accounting, not only for the dark colour of the blood, but for most of the other phenomena in cholera. This opinion has been supported in modern times by Dr. George Johnson,[c] who ascribes not simply the dark colour, but also the defective coagulability of the blood, to want of oxygen. There is, however, no satisfactory evidence to shew either a deficiency of oxygen, or an excess of carbonic acid, in the blood of cholera patients during collapse; and as there is no mechanical obstacle to the free entrance of air by the lungs, and as the inhalation of even pure oxygen is attended with no improvement, it may even be inferred, that so far from a deficiency of the one or an excess of the other occurring in cholera, that their relative proportion is always maintained during collapse, except in those cases in which the condition (collapse) of the lungs after death may appear to justify the inference, that the amount of carbonic acid in the blood is relatively diminished, and oxygen therefore present in slight excess.

---

[a] "Sketches of the most prevalent Diseases of India," 1825, p. 142.

[b] Dr. Turnbull Christie, whose opinion respecting the nature of cholera appears to be more nearly correct than that of almost any other writer on the disease, has fallen into this error, in stating that "the

It has been suggested by Dr. Stevens,[d] that the original colour of the blood is black; that the red is due to the salts; and that even arterial blood becomes

---

black colour of the blood is probably owing to its not being decarbonized in its passage through the lungs; the air being prevented from acting upon it by the diseased secretion thrown out by the pulmonary mucous membrane." ("Observations on the Nature and Treatment of Cholera," Edinb. 1828, pp. 82-3.)

[c] "On Epidemic Diarrhœa and Cholera," 1855, p. 207.

[d] "Observations on the Healthy and Diseased Properties of the Blood," 1832.

dark if the salts are washed out. "When the salts," Dr. Stevens observes, "are lost, or greatly diminished, as in the last stage of yellow fever, in cholera, etc., the blood becomes black, exactly in proportion to the diminution of its saline matters." It may be objected to Dr. Stevens' theory, that although washing out all the salts causes the blood to become dark, yet the first effect of washing the blood is to render its colour lighter; which, as some of the salts are removed by the process, is irreconcileable with the statement, that absence of the salts is the cause of the dark colour of the blood. For if removal of all the salts causes the blood to become black, a removal of part of the salts ought certainly to darken instead of lighten its colour.

It may be observed, in connection with the colour of the blood in cholera, that electricity and galvanism appear to possess great influence over its change. Dr. Wilson Philip passed a galvanic current through arterial blood, and its colour changed from red to black and heat was evolved. Sir Everard Home[e] observed, that blood exposed to the action of the negative pole of a galvanic battery becomes black, fluid, and alkaline to a high degree; whilst at the positive pole it coagulates, becomes red, and acid. These observations are unsatisfactory, as the changes noticed may be the result of some of the constituents of the blood being decomposed by this agency; and they can therefore be considered as offering a very doubtful explanation of what may take place in cholera. The more recent experiments of M. Brown-Séquard,[f] on the influence of galvanism on the colour of the blood, are more satisfactory, and less liable to this objection. M. Brown-Séquard observed, that after dividing the nerves of a limb, the change in the colour of the blood, from red to black, is effected with difficulty; but that the change takes place very readily if the paralysed limb is galvanized. This observation of M. Brown-Séquard, if correct, is conclusive against the supposition of diminution of cerebro-spinal nervous influence being the cause of the dark colour of the blood in cholera.

---

[e] "Phil. Trans." 1809, p. 388.

The view that I am disposed to take of the cause of the dark colour of the blood in cholera, is the same as that which was advanced some sixty years ago by Dr. Wells,[g] to explain the nature of the change effected in the colour of the blood in health. According to Dr. Wells, the change of colour is the result of physical causes, affecting "the form of the blood corpuscles, and their consequently different modes of reflecting and transmitting light." Dr. Wells further observes, that "the exterior part of the globule appears to be that ingredient of the blood, upon which common air and the neutral salts produce their immediate effect when they render the whole mass florid." If, as Liebig has since more forcibly suggested, the blood corpuscles should be regarded as the principal carriers of oxygen to the system, and of carbonic acid to the lungs, it may be assumed that the difference in the colour of arterial and venous blood, results from the different specific gravities of the oxygen and carbonic acid which they respectively convey; and that if the oxygen which enters the blood in the lungs, were of the same specific gravity as the carbonic acid which it replaces, no change would take place in the colour of the blood as a consequence of respiration.[h] The dark colour of the blood in cholera, admits of being readily explained by this theory, which also satisfactorily accounts for the absence of coma, hitherto looked upon as an almost necessary consequence of the prolonged circulation of dark blood. As the fluid medium, in which the corpuscles of the blood in cholera float, increases in density whilst it diminishes in quantity, so does the colour of the blood become darker; until at last the physical changes in the liquor sanguinis are carried so far, that even the interchange of carbonic acid and oxygen in the lungs, is insufficient to effect any material alteration in the form of the corpuscles, and the blood consequently retains a dark colour in the arteries; notwithstanding that it has been fully subjected to the respiratory change, and has acquired as much oxygen in proportion to the amount of carbonic acid it had to part with, as on ordinary occasions.

---

[f] "Comptes Rendus des Séances de la Soc. de Biol." 1852, p. 4.

[g] "Observations and Experiments on the Colour of Blood."—Phil. Trans. 1797.

---

[h] The exact nature of the change affecting the form of the corpuscles seems to be still the subject of dispute. Dr. Wells in ascribing the red colour of arterial blood to increased reflection of light by the corpuscles, observes, that this "must have arisen from some change in their internal parts, by means of which much of the light which had formerly been suffocated, was now sent back through their anterior surfaces, tinged with the colour of the medium through which it had passed." Mulder, and others, have since affirmed, that the difference of colour in venous and arterial blood, is due to the venous corpuscles being bi-convex, and the arterial bi-concave. I must forbear, however, to enter into any discussion concerning the relative merits of the different statements which have been made on this subject, as it would not, apparently, assist in removing any of the difficulties connected with the nature of cholera.

(*c*).—*On the coagulation of the blood in cholera.*

As the spontaneous coagulation of the blood, under all circumstances of health and disease, depends not alone on the amount, but also on the condition of the fibrine which it contains, it may be alleged that in cholera, any diminution, or loss of such property, should be ascribed to causes affecting this element of the blood. Now, it has been ascertained, that, in health, venous blood, which besides other points of distinction, contains less fibrine than arterial blood, does not coagulate so readily as the latter; and that in death from asphyxia, in which the venous character of the blood is most marked, it frequently retains its fluidity. It has also been observed that the blood of the vena porta contains less fibrine, and coagulates more slowly than any other venous blood. These facts have been applied to account for the same altered property of the blood in cholera, which some seem disposed to think, differs from healthy blood chiefly in possessing an exaggerated venous character. There is, however, as I have already endeavoured to show, no real connection between them; for, except the resemblance in colour, and occasionally also in not coagulating, the blood in cholera and the blood in asphyxia are essentially distinct. The cause of the resemblance in colour may be referred, as before stated, to the same physical effect being produced by different means; and the non-coagulability of the blood can in like manner be traced to the tendency manifested by apparently opposite causes to produce corresponding results.

The coagulation of the blood may be prevented by either the sudden or the gradual operation of causes, acting directly on the blood itself, or through the intervention of the nervous system: the effect being produced either by the presence of a material poison in the blood, or by an impression communicated to the blood, through the medium of the nerves distributed to the blood-vessels.

In asphyxia, it is evident that the accumulation of carbonic acid acts directly as a poison on the blood, and in this way destroys certain of its properties;[a]

---

[a] For a notice of the effects of carbonic acid in preventing coagulation of the blood, consult Hewson's Works, edited by Gulliver, (pp. 68, 69). In some cases mentioned by Mr. Gulliver, the blood, forty-eight hours after death from asphyxia, was fluid and did not subsequently coagulate. Mr. Gulliver, in reference to these cases, states "the fingers were contracted, and all the limbs rigid:" showing that rigor mortis is no evidence of the blood being coagu-

and in many other diseases, as small-pox, etc. the blood appears to be poisoned, in consequence of the introduction of extraneous matter, possessing the character of a ferment. In accordance with these observations, it has frequently been alleged, that in cholera the blood is contaminated either by the retention of effete matters, which are ordinarily got rid of by the excretory organs; or by the introduction of a specific blood-poison, admitting of almost unlimited development. Notwithstanding the great amount of support these opinions have always received, and the distinguished position of many of their advocates, it does not appear to me that there are any satisfactory reasons for supposing that the blood is poisoned in cholera. There is no evidence to prove, that any of the effete matters ordinarily present in the blood, as carbonic acid and urea, are accumulated in it, previous to or during the collapse; and, with reference to the presence of a specific organic poison in the blood, the arguments against the supposition of cholera being caused by this means, seem to be almost conclusive.

In all undoubted cases of blood-poisoning, from the introduction of an organic matter capable of acting as a ferment, the character of the resulting disease, except where death occurs suddenly, is manifested by a distinctive eruption on the skin; and the

---

lated; an argument which has been often employed by those who deny that certain violent modes of death, have a tendency to prevent coagulation of the blood.

disease itself admits of being communicated to other individuals by inoculation, either with the blood of the patient, or the morbid matter localized in the skin. In cholera, on the contrary, if any specific poison be present in the blood, it does not give rise to any peculiar eruption on the skin, even in protracted cases of the disease:[b] whilst inoculation with the blood of cholera patients has been ascertained by different observers to fail in reproducing cholera. It may, perhaps, be urged, that the absence of an eruption on the skin in cases of cholera, may be accounted for by the occurrence of a characteristic flux from the alimentary canal, and that the system endeavours to relieve itself of the poison by means of the alimentary mucous membrane in preference to the skin. The well-known relation or sympathy between the skin and the alimentary mucous membrane, would certainly justify the inference, that the morbid matter in the blood may be determined to the one surface instead of to the other. But this argument in favour of the blood being contaminated in cholera, however plausible it may seem, cannot be received as correct. There is no experimental proof, as already stated, of the blood containing a specific poison; whilst the characteristic discharges in cholera have been repeatedly swallowed with impunity, and have also been introduced into the system by inoculation, without inducing the disease.[c]

---

[b] The eruption described by Dr. B. G. Babington ("Med. Gaz. 1832," p. 578.) seems to have been a species of Urticaria; and occurring, as he states, after typhoid consecutive fever had existed some days, is indicative of gastro-intestinal irritation. Consequently, an eruption of this character tends to support the hypothesis which refers the disease to the gastro-intestinal mucous membrane. Dr. Keir ("Treatise on Cholera," p. 68) who had previously pointed out that cutaneous eruptions sometimes occur when reaction has been established, ranked them among "the critical efforts of nature in this disease." It is probable that the rash in these cases is similar in its nature, and also in its mode of origin, to that which sometimes follows the eating of shell-fish, and the drinking of iced-water in summer.

---

[c] There are several instances on record in which the characteristic evacuations of cholera have been tasted and swallowed, either purposely or by mistake, and have failed to produce any specific effects. M. Foy, in the presence of several witnesses, tasted the matters vomited by a cholera patient at Warsaw, in 1831, and received no harm in consequence. ("Lancette Française," Juillet 2, 1831; and "Histoire Médicale du Choléra-Morbus de Paris," 1832, p. 44.)

Mr. Simon M'Coy, one of the resident medical officers of the Grange Gorman-lane Cholera Hospital, Dublin, states, that "he tasted the rice-vomit and escaped." ("Dublin Journal of Med. 1833." p. 360.)

M. Schmidt relates that a drunken man swallowed half of a beer-glassful of the matters vomited by a cholera patient, and received no harm. ("Characterisk der Epid. Chol." p. 81.)

Messrs. Pearse and Marston state, "that one of the dispensers drank (by mistake) some rice-water evacuation without any injurious effects whatever." ("Med. Times and Gaz." Feb. 25, 1854.)

Mr. Moir, one of the most uncompromising advocates in favour of contagion, cites the following evidence of Dr. Becker on the subject. "On the 5th of September," says Dr. Becker, "a cholera dissection was performed in one of the streets near the river (Schleuse), which had been the seat of the first and numerous successive cases of the disease. Four young physicians present, not satisfied with the information derived from the senses of sight, touch, and smell, thought proper to ascertain the properties of the blood, and contents of the intestines, by tasting these fluids. One of these gentlemen, Dr. C., one of the loudest adversaries of contagion, before and since the appearance of cholera at Berlin, had for a fortnight laboured under diarrhœa, but continued his professional avocations. On the

If, therefore, the poison in cholera be determined to the surface of the alimentary mucous membrane, and escape in the characteristic discharges from the stomach and bowels, it may be asked, "Why is there no conclusive evidence of its presence

---

7th he again attended a cholera patient in the evening; on the morning of the 8th he was attacked with cholera, and died that evening.

"Another of the physicians mentioned above, Dr. J——, accompanied me to Dr. C——'s sick-bed; we saw him half an hour before his death. I shall never forget the words he said, with the suppressed voice so peculiar to cholera patients:—'Ah, Dr. J——, beware! let my fate be an example to you!'"—("Proofs of the Contagion of Malignant Cholera." By D. M. Moir, 1832, p. 35.)

This evidence of Dr. Becker, so far from being in favour of contagion, appears to be almost conclusive against the supposition of cholera being, under any circumstances, developed by the contact of cholera evacuations with the lining membrane of the alimentary canal. Only one out of the four physicians, who performed the experiment, died from cholera; and the one who died is acknowledged to have had diarrhœa for a fortnight, and to have neglected himself.

All experiments of this description should be regarded, however, as unsatisfactory, for cholera may occur co-incidently, and not as a necessary consequence of tasting or swallowing the discharges. I am inclined to consider these experiments as more disgusting than they are dangerous, provided the matter taken be not at the time in a state of decomposition. For, as I have stated in the text, decomposing animal or vegetable matter of any kind is a powerful exciting cause of the disease. These observations agree with the results of the experiments of Mr. Marshall ("Brit. and For. Med. Chir. Rev." 1853); and Dr. Lindsay ("Edinb. Med. and Surg. Journ." 1854), on the communicability of cholera to the lower animals.

Dr. Tytler ("Remarks upon Morbus Oryzeus," Calcutta, 1820, pp. 81, 82) appears to have succeeded in proving too much, when he ascertained that by feeding a goat on bad rice, death from cholera occurred; the animal having been "attacked with every characteristic symptom of the disease, affecting the human species."

in them?" As neither the blood, nor the flux, from cholera patients, seems to be capable of reproducing the disease, the hypothesis of a specific blood-poison is inadmissible, and the altered properties of the blood must consequently be ascribed to some other cause.

In the absence of any satisfactory evidence of the blood being poisoned in cholera, its non-coagulation may be assumed to be the result of some affection of the nervous system; and in favour of its dependance on the sympathetic, it may be stated, that this hypothesis coincides with what has been observed respecting the occasional non-coagulation of the blood in certain violent modes of death, which admit of being more directly referred to the sympathetic nervous system, as in death from lightning, electricity, blows on the stomach, etc.; in all of which the blood has not unfrequently been observed after death to retain its fluidity.[d] It accords also with the imperfect, and sometimes altogether interrupted coagulation of the blood in those diseases which result from abnormal nutrition, as scurvy and purpura; which appear to be consequent, not simply on the bad quality of the food which supplies fresh

---

[d] The statement, that the blood retains its fluidity after certain violent modes of death, rests chiefly on the authority of Hunter, who observes, that "in many modes of destroying life the blood is deprived of its power of coagulation, as happens in sudden death produced by many kinds of fits; by anger, electricity, or lightning; or by a blow on the stomach, etc. In these cases, we find the blood, after death, not only in as fluid a state as in the living vessels, but it does not even coagulate when taken out of them" ("A Treatise on the Blood," etc., 1794, p. 26). The accuracy of this observation has been a frequent subject of dispute, and is likely to continue so, until some more definite information has been collected respecting the general condition of the blood in these cases. Of the objections which have been from time to time urged against it, I would now only refer to two, which are all that appear to be important, viz., that coagulation of the blood in these cases is not prevented, but

---

delayed; and that it is not prevented, because clots have been found in the heart. The first of these objections applies to many cases of alleged non-coagulation, in which the blood after death has retained its fluidity for an unusually long period, but has subsequently become coagulated. It must be admitted, that such an occurrence would be very likely to give rise to the hasty and, as some may think, erroneous conclusion, that the blood had lost its property of spontaneous coagulation; but at the same time, it also appears to prove, that although the blood has, after a long interval, become coagulated, it must have undergone some unusual change, or have been subjected to some peculiar influence, before its most characteristic property could have been thus suspended. It may be, indeed, that the cause which determines the coagulation after this unusual delay, differs from that which effects it under ordinary circumstances. —With reference to the second objection, it may be urged, that the presence of clots in the heart, is insufficient to prove that the blood is spontaneously coagulable; as these clots have been not unfrequently observed in connection with a permanently fluid state of the blood elsewhere. In the following case, recorded by Mr. Gulliver, of a hare "that had been run for thirty-five minutes, and then killed by the Windsor harriers," it will be observed that this was the case, and also that the condition of the fibrine in the effused blood was similar to that which is met with in the cholera evacuations. The animal, Mr. Gulliver relates, "was immediately gutted, and I examined it four hours afterwards. After pressing the blood into the jugular veins, portions of them were removed and laid aside with the contained blood; it never coagulated. There were some small but distinct clots in both ventricles of the heart, all the cavities of which were otherwise empty. In the left pleura there was upwards of a quarter of an ounce of thick yet fluid blood, which never clotted, but in which, when mixed with water and filtered, some small fibrinous flakes were found."—"Hewson's Works," edited by Gulliver, p. 21.

material to the blood, but also on the morbid character of the changes effected in the blood, by the irregular and defective action of the nerves distributed to the blood-vessels, and which supply the stimulus by which those changes are effected. Lastly, it agrees with the information derived from the leading symptoms in cholera, and it contributes to explain the origin of the flux from the alimentary canal. If consequently it should be allowed that the defective coagulability of the blood in cholera may be the result of some affection of the sympathetic nervous system, it remains to be considered in what way the condition of the blood can be so altered, as to lose its property of becoming solid when removed from the influence of the vessels, in which, during life, it is maintained in the fluid state.

The opinion commonly entertained, that the spontaneous coagulation of the blood is an evidence of the inherent vitality of its fibrine, and which is thought to be confirmed by its capability, under certain circumstances, of becoming organised, does not appear to be well-founded. Fibrine, in the solid state, never becomes organised except in contact with some of the living tissues of the body; and although it has been supposed that the stimulus, which it evidently derives from this source, is not required to impart vitality, but only to develop that which is already present in the fibrine in a latent form, yet its organization may with equal, if not greater, probability be referred to the development of the colourless corpuscles, which are always associated with the fibrine, and are present in greatly increased proportion in the fibrine employed in the process of reparation, and in the inflammatory exudations on serous membranes, which have been observed to be very prone to become organised.

The statement, that the fibrine of the blood in health is maintained in solution by contact with the parietes of its own vessels, would appear to be proved by the circumstance, that when it is removed from such influence it passes into the solid state; although the time required to effect this change may, in the event of the blood being retained within the body, be very prolonged. In addition to this, it is very commonly alleged, that although contact with the parietes of the vessels in which it is contained is the chief cause of its fluidity, yet that motion is also necessary, and that when the circulation of the blood is interrupted, as by ligature of arteries, or impeded, as in aneurisms, that the fibrine is apt, in consequence, to become solid. That the fibrine, under these circumstances, does pass from the fluid to the solid state, is not conclusive evidence, however, in favour of motion being necessary to maintain its fluidity; or that contact with the parietes of its vessels might not, in health, be alone sufficient for that purpose. In both of these apparently exceptional cases, the condition of the blood-vessels has undergone a change, and the blood in them is no longer subject to the customary nervous influence.

In the one case, where the artery has been ligatured, and the inner coats of the vessel have in consequence been divided, the influence of the sympathetic nerves distributed to the vessel must at that part be to a certain extent interrupted; and the blood, like the contents of the stomach after perforation, ceases to be influenced, as usual, by the tissue with which it is in contact. In the other case, where aneurism has occurred in consequence of accident or disease, causing lesion of one or more of the coats of the artery, the condition of the vessel, as far as the influence of its nerves is concerned, is to some extent the same as after the application of a ligature. But the coagulated fibrine in an aneurismal sac generally differs from that in an artery which has been ligatured, in exhibiting little or no disposition to become organised, which may partly be due to deficiency or absence of the colourless corpuscules, consequent on the mode in which the clot in this instance has been formed. As the circulation of the blood can, in neither of these cases, be said to be affected previous to the vessels themselves receiving such an amount of injury, as would be capable of causing local interruption or disturbance of the nervous action, the influence of motion in maintaining the fibrine in a fluid state should be at least doubted, if not rejected.[e]

It may appear, perhaps, somewhat paradoxical to assert that the physical condition of the blood is dependent on the nervous influence of the vessels, when the fibrine is observed to become coagulated in the two preceding cases, and to remain fluid after death from cholera, electricity, blows on the epigastrium, etc.; which have been ascribed in like manner to diminution or interruption of the sympathetic nervous influence. But this apparent anomaly will disappear, when it is considered, that in the one class of cases the greater mass of blood is altogether unaffected, and that the local coagulation takes place in consequence of the blood being arrested, and kept in contact at that part with a texture which has no longer any special power over it. Whilst in the other class of cases, the impression on the sympathetic nervous system having been sufficient to diminish, to a greater extent, the nervous power in the blood vessels, the relation between the blood and its vessels is no longer the same as in health, and the blood either undergoes some change in its constitution, by prolonged contact with the semi-nervous parietes of its vessels, or some portion of the impression may be directly communicated to the blood itself, and have the effect of at once altering its condition, so as to prevent coagulation, without at the same time effecting any change in its chemical composition. In the first of these conditions, where the influence of the sympathetic nervous system is diminished, the death would be gradual; but in the second, where its influence is destroyed, the death would be sudden.

---

[e] "Rest does not of itself in the least assist the coagulation of the blood."—"Hunter on the Blood," 1794, p. 25.

The power of the nervous system in thus preventing the coagulation of the blood, may be compared to the effects of moderate heat on albumen, which after being dried at a temperature not exceeding 126° Fahr., retains its solubility in water, and the solution may be subsequently exposed to a temperature of 212° without coagulating. If heat be able to effect this change in the one instance, the nervous system may be allowed to possess an equal influence in the other; and as heat and nervous action have been regarded as modifications of the same power, so albumen and fibrine may, from their apparently equal susceptibility to these allied forces, be regarded as modifications of the same substance.

With reference to the coagulable principle of the blood, whether it should be considered as formed direct from the fluid albumen, or from the waste materials of the animal tissues, its condition in cholera appears to be regulated by the extent to which the sympathetic nervous action is disturbed; and as there is sufficient evidence to conclude that the usual products of the organic functions in this disease, either cease to be or are very imperfectly developed, it is probable that the conversion of albumen or other organic matter into fibrine, may be also imperfect; and that what would otherwise, in a healthy state of the body, be fibrine, remains in a transition state, ready to assume the ordinary character of fibrine, as soon as the necessary force or agent is applied for that purpose. Whether this explanation be thought sufficient to account for the defective coagulability of the blood in cholera, or whether, in addition, the fibrine supposed to be previously present in the blood of cholera patients, does not undergo some change of re-construction, and in consequence lose its property of spontaneous coagulation, it is certain that fibrine and albumen are very closely allied together, and if not allotropic conditions of the same substance, are, according to Liebig, the same in the relative proportion of their elements.[f]

This close relationship between fibrine and albumen may be still further illustrated by the fact of their being readily convertible the one into the other. The conversion of fibrine into albumen was first demonstrated by M. Dénis, who succeeded in imparting to fibrine the solubility in water, and the coagulability by heat, which distinguish albumen. Whilst the conversion of albumen into fibrine has been proved by an interesting series of experiments by Dr. Andrew Buchanan, "on the coagulation of the blood and other fibriniferous liquids."[g] Dr. Buchanan found that the liquid of hydrocele, and other dropsical liquids, not spontaneously coagulable, could be made to coagulate by introducing into them a portion of some of the animal matters, as washed blood-clot, substance of spinal marrow, muscular fibre, etc.; and that a corresponding effect was produced when different kinds of serum were mingled together, as the serum of hydrocele and the serum of blood, each having been previously proved to be uncoagulable spontaneously. The conclusions Dr. Buchanan draws from his experiments (which have been repeated and confirmed by Mr. Gulliver[h]) are, "that fibrine has not the least tendency to deposit itself spontaneously in the form of a coagulum: that, like albumen and casein, fibrine only coagulates under the influence of suitable reagents; and that the blood and most other liquids of the body which appear to coagulate spontaneously, only do so in consequence of their containing at once fibrine and substances capable of reacting upon it, and so occasioning coagulation." The reagent which effects the coagulation of the blood is, according to Dr. Buchanan, the colourless corpuscles, to whose power of self-development I have already ascribed the organization, under certain circumstances, of a fibrinous clot; and the reasons assigned for that opinion may be applied in support of Dr. Buchanan's view of the formation of the clot.[i]

---

[f] "Animal Chemistry," edited by Gregory, 1842, p. 42.

[g] "London Medical Gazette," 1845, pp. 617—620; see also a paper by the same author, entitled "Contributions to the Physiology and Pathology of the Animal Fluids."—"London Medical Gazette," 1836.

Whilst, therefore, the non-coagulation of the blood in cholera is in itself no evidence of contamination, as the blood may be poisoned and yet retain its coagulability; the occasional absence of this property of the blood in cholera coincides with the theory which refers the disease to an affection of the sympathetic nervous system; which, as it supplies the nerves distributed to the blood-vessels, must consequently convey to the blood the impression of any external influence capable of altering its character. The readiness with which albumen can be made by the presence of a catalytic (?) agent, to assume the character of fibrine, justifies the inference that fibrine does not exist as such in the blood. But if the experiments by Dr. Buchanan should not be thought to warrant this inference, it must be allowed that the fact of chemists being able to convert fibrine into albumen favours the supposition, that in cholera and other diseases, the blood may be deprived of its property of spontaneous coagulation, with coresponding facility, by the operation of some organic influence.

---

[h] "Hewson's Works," edited by Gulliver, p. 31.

[i] One of the most distinguished of modern physiologists (Dr. Carpenter) in adopting the opinion, that the office of the colourless corpuscles is to convert albumen into fibrine, does not apparently consider them as in any way connected with coagulation. This property of the blood seems indeed to be very generally regarded as "one of those ultimate facts in physiology, which we must be content to observe and to describe, but of the cause of which we are likely to remain ignorant." ("Phys. Anat. and Phys. of Man," by Drs. Todd and Bowman, vol. ii. p. 294.)

Before concluding this division of my subject, it may be asked, what is the position of the blood in cholera?

From a careful consideration of the foregoing remarks, I would infer that the blood within its vessels is partly in the same position as it would be if removed from the body: that is to say, that contact with the parietes of its vessels is no longer attended with those effects which are necessary to a due performance of the functions of life. The blood-vessels, under these circumstances, may be compared to an electric machine at rest. Power may be communicated or excited by artificial means, as by galvanism, by the stimulus of saline injections, etc.; but the changes consequent on a restoration of function will, under these circumstances, be only temporary; and the vessels in relapsing into their former powerless state, will leave the blood to that rest which is inconsistent with life, or to the influence of another kind of action which causes death.

3. *The flux from the stomach and bowels.*

The position which should be assigned to vomiting and purging, as symptoms of cholera, has always been the subject of much dispute, and has led to many errors which still continue to affect both the pathology and the treatment of the disease. The insidious way in which cholera usually begins with diarrhœa, and the peculiar character and often large amount of the subsequent flux from the stomach and bowels, have induced most observers to attach great importance to these symptoms; and many writers have assigned to them what appears to be an undue prominence over the rest, by speaking of them as the essential cause of the collapse, and the chief guide for treatment. Their claim, however, to be so considered, may be in many cases of cholera disputed, and in some unhesitatingly denied. All who have, like myself, had much experience of the disease in tropical climates, must have felt convinced that the amount of the discharges in cholera was sometimes quite inadequate to account for the collapse, and that some other cause was more directly concerned in producing the fatal result. I admit that the disease usually appears to commence with diarrhœa, especially in temperate climates; and that in consequence of this early occurrence of diarrhœa, there is, in most cases of cholera, sufficient time allowed for the successful application of remedies, if we would only attend to the warning which nature gives us. The importance of this premonitory diarrhœa was fully recognised in this country when cholera first made its appearance; and the necessity of attending to it was forcibly impressed on the public mind, in the following heading to the "Form of Handbills recommended for circulation in the infected districts," and published January 26th, 1832, by the Central Board of Health.

"CHOLERA DISTRICTS."

"Looseness of the Bowels is the beginning of Cholera."

The attention which has been directed of late years to the same subject by Dr. Macloughlin, cannot be too warmly commended; for there appears to have always been a general tendency in the public, and even among many members of the medical profession, to neglect this state of the bowels during the prevalence of cholera, and many lives have probably been sacrificed in consequence of this suicidal apathy. But whilst acknowledging the great importance which should always be attached to this symptom during an epidemic of cholera, I must object to the exclusive dogma, that cholera is always preceded by diarrhœa; for there are undeniable cases on record, of patients who have died from cholera without having had one cholera evacuation from either the stomach or the bowels. From my own experience of the disease, I believe that in all the more suddenly severe cases of cholera, especially in those which I have had the opportunity of observing in India, a notable failure of the circulation always precedes the diarrhœa. But as this is not easily recognizable by the public, it cannot be regarded as so useful a signal of the approach of cholera as is the so-called premonitory diarrhœa. Before, however, proceeding further to investigate the connection in cholera, between the flux and the collapse, it will be necessary to consider the characters of the fluid discharged from the alimentary mucous membrane, and the probable cause of the flux.

(*a*). *The characters of the fluid discharged from the alimentary canal.*

The discharge from the stomach and bowels in cholera consists generally of a semi-transparent whitish fluid, in which numerous greyish particles float; and it resembles more a mixture of gruel and water, than the rice-water to which it is customary to compare it. Much difficulty must always attend any efforts to determine the amount of the fluid lost; and even if the quantity discharged could be ascertained with greater facility, it would not probably lead to any very useful results, as there are many circumstances which tend to modify its effects on the system; such as the previous condition of the patient, the intensity of the attack, and the rapidity of the discharge. In one of the cases observed by Dr. Parkes, in India, the amount of the discharges was satisfactorily ascertained, and found to be 88 oz. In reference to this point, it may be remarked, that the amount of the flux appears to vary considerably, not only in different individuals, but also in different epidemics. According to Drs. Russell and Barry,[b] it would appear that the vomiting and purging were not so copious or so frequent in the Russian as in the Indian epidemic of cholera (which Dr. Russell had had the opportunity of personally observing); and they were far from being looked upon as the most important or the most dangerous symptoms, as they generally ceased or were arrested early in the attack: whilst in the late epidemic of cholera in this country (1854), the average amount of fluid discharged in each case was very considerable, and decidedly more than I have witnessed in the cholera of India and China.

This fluid possesses a low specific gravity (1.008); alkaline reaction; and not unfrequently there is noticeable in it, a peculiar odour resembling stale blood. From the trustworthy paper by Dr. Parkes,[c] on the analysis of the intestinal discharges in cholera, it appears that 1000 parts of the fluid contain 13.10 of solid matter, which consists of:—

---

[b] "Official Reports on Cholera in Russia," 1832, p. 39.

Coagulable organic matter, albumen	0.29
Incoagulable organic matter and insoluble salts .	6.82
Soluble salts, viz. chlorides, phosphates, and sulphates	5.99
	13.10

The solid organic matter, which may be separated by filtration through fine gauze, is now generally considered to be fibrine, or some modification of that compound. This was early recognised by Dr. Christie,[d] who states that both the coagulated part of the discharges, as well as the pultaceous matter often found lining the mucous membrane, consists of fibrine. Dr. O'Shaughnessy states as the result of his analysis in the case of a cholera patient (Dewar), in whom "the appearance of these evacuations was most marked," that he was "inclined to conclude that the flaky matter in this case was principally composed of fibrine."[e] Corresponding results were obtained by Dr. Parkes, who, from a series of analyses of the cholera evacuations, regards "all the flocculent matter of the true cholera stool, cells, dark yellow granules, fibres, flakes, and amorphous matter, to be but modifications of the same substance, viz. fibrine."

---

[c] "London Journal of Medicine," 1849, pp. 134-152.

[d] "Observations on the Nature and Treatment of Cholera," Edinburgh, 1828, pp. 52 and 75.

[e] "Report on the Chemical Pathology of the Malignant Cholera," 1832, p. 66.

Two important questions occur in connection with the saline constituents of the cholera evacuations, which require special notice; viz. 1st, Is urea, or its equivalent carbonate of ammonia, present in such quantities in the cholera evacuations, as to justify the inference, that it is excreted, during collapse, by the alimentary mucous membrane instead of by the kidneys? and 2nd, On what salt does the alkalinity of the cholera evacuations principally depend?

1. As urea has been very often alleged to be got rid of in cholera by the flux from the stomach and bowels, and as this opinion still continues to be commonly entertained, it is desirable to determine, if possible, whether there is any chemical evidence to support such a hypothesis. From the results obtained by different enquirers, the evidence appears to be almost conclusive against the supposition of urea being present in the cholera evacuations; or if so, of being present only in such small quantities, as not to account for its non-secretion by the kidneys. Dr. O'Shaughnessy, who appears to have been very careful in his analysis of the cholera evacuations in the case of Dewar, already referred to, failed altogether to detect urea. Whilst Dr. Parkes, who has directed particular attention to this subject, failed to detect it in any quantity in the cholera evacuations he examined.

The hypothesis, advocated by Dr. Wood and others, that the urea is vicariously secreted by the alimentary mucous membrane and afterwards decomposed into carbonate of ammonia, must be regarded as untenable. For if urea had been so eliminated from the system, there would always be some evidence of its presence in the flux, unless its decomposition was coincident with its excretion; a supposition which is not only in itself improbable, but is contradicted by the chemical analyses of the evacuations, which shew that carbonate of ammonia is not among the usual constituents of the flux.

These negative results, which are all that we should expect to obtain, coincide with the theory which I am endeavouring to illustrate, and which presupposes that the formation of urea in the system is more or less completely interrupted during the continuance of collapse.

2. With reference to the alkaline reaction of the cholera evacuations, it has not been satisfactorily determined on what salt this property chiefly depends; but the fact of the characteristic discharges in cholera being alkaline is almost undisputed. Nearly all observers, including Foy, Vogel, Rose and Wittstock, O'Shaughnessy, Parkes, and others, agree in stating that the reaction is alkaline, and some of them state that it is even strongly so. For example, Vogel affirms that "the fluid has a powerful alkaline reaction."[f] Similar evidence is also afforded by MM. Rose and Wittstock, who made repeated observations on cholera patients in the hospitals at Berlin, during the winter of 1831-2, and found that the dejections

---

[f] "Simon's Animal Chemistry," edit. by Day, vol. ii. p.382.

were "strongly alkaline."[g] Dr. O'Shaughnessy ascertained, that "turmeric paper immersed in the fluid was strongly reddened, and reddened litmus paper restored to its blue tint."[h] So also in some of the observations reported by Dr. Parkes,[i] the alkaline reaction was very decided. It may therefore be admitted that the characteristic discharges from the stomach and bowels in cholera are alkaline, and in some cases strongly so.

I have been thus particular in showing that the cholera evacuations are alkaline, because I regard this fact as one of the greatest importance in the present enquiry; and a correct interpretation of this phenomenon will probably furnish us with the key which will unlock the nature of the disease.

It is not yet known, as I have already stated, to what salt the alkalinity of the flux is due. In most of the analyses which have been performed to determine the amount of the salts, the quantity of each has not been separately ascertained. According to Dr. O'Shaughnessy, the soluble salts contained in the flux are the muriate, acetate, and carbonate of soda. As the salts left, after evaporating the water, were calcined by Dr. O'Shaughnessy, it is probable that much of the carbonate of soda may have existed previously in some other form than that of carbonate, and that a portion at least of this salt was formed during the analysis. The inference which might be deduced from the results of Dr. O'Shaughnessy's analysis, must therefore be received with considerable doubt, as far as the nature of the salts is concerned. According to Dr. Parkes, the salts usually present in the cholera evacuations, are the chlorides, phosphates, and sulphates; but in most of the analyses he has detailed, the quantity of each has not been separately determined. In one of the cases related by Dr. Parkes (Observ. 8), where this was partly done, the principal alkaline salt present was the phosphate of soda, which is stated to have amounted to 1.8 in 1000 parts of the flux, or nearly one-third of the soluble salts present; and as there was also present 1.2069 "of other soluble salts, including carbonates," it is probable that the quantity of phosphate of soda originally present in the fluid may have been even still greater. However, the amount of phosphate of soda in this case, as determined by Dr. Parkes, is very large; and when it is remembered how greatly it exceeds the relative quantity of the same phosphate contained in the blood, it may be assumed that the salt which produces the alkalinity of the flux is the same as that which causes the alkalinity of the blood, and that it is owing to this salt being abstracted from the blood in greater proportion than some of the other saline constituents, that the blood becomes neutral and even acid in those cases in which the flux is profuse. This statement seems to be conformable to the general results obtained by Dr. Parkes, as in all the observations he has reported, soluble and insoluble phosphates are said to have been present, and in some cases to a considerable extent. It would, however, be desirable to have more exact information on this subject, which appears to me of greater importance than the researches to determine the precise character of the flocculent matter contained in the flux, and which can have but little practical value in an enquiry concerning the nature of cholera; for it is not of any great consequence to know whether the latter consists of albumen, fibrine, or some intermediate organic compound; whereas, by ascertaining the cause of the alkalinity, we should be enabled to determine, with greater certainty, the cause of the flux.

---

[g] "Gaz. Med. de Paris," Janvier 14, 1832, p. 21.

[h] "Report on the Chemical Pathology of the Malignant Cholera," 1832, p. 65.

[i] "Lond. Med. Journal," 1849, pp. 134-152.

(*b*). *The probable cause of the flux.*

The post-mortem appearances in cases of cholera are generally considered to afford no direct evidence of the cause of the flux from the alimentary mucous membrane. It is true that some writers on the subject have directed attention to certain pathological states of the alimentary canal, and have endeavoured to establish a definite connection between such appearances after death and the symptoms during life. Annesley attached much importance to a finely injected state of the small intestines, which he considered "to be peculiar to this disease, and belonging to its pathological character."[s] A corresponding condition of the mucous membrane both in the stomach and intestines, was particularly noted by Orton, who remarks that "the degree in which these appearances in both stomach and intestines existed, seemed in general to bear some relation to the duration of the case, for in such as had been of several days' continuance they were highly marked."[t] This injected state of the mucous membrane has been frequently observed in this country and elsewhere in Europe; but as it is not unfrequently absent, especially in those cases which prove quickly fatal, it can be but of little assistance in explaining symptoms which may occur independent of its presence. I should be more disposed to regard this pathological condition of the alimentary mucous membrane as the result rather than the cause of the cholera flux.

---

[s] "Sketches of the most prevalent diseases of India," 1825, p.37.

Inflammation of portions of the mucous membrane of the alimentary canal, has been frequently observed in connection with cholera; but this also appears to have been generally a secondary affection, and it cannot, therefore, be considered as the cause of the flux. In the first reports on cholera in the three Presidencies of India, it is stated that the post mortem appearances of the alimentary canal were found to be such as could afford only negative evidence on the subject. The appearance of inflammation was not only absent in a very large number of the cases examined, but in some an opposite condition of the alimentary canal was occasionally noticed. Thus in the Bengal Presidency, Jameson states that "in the bodies of those who sunk at a very early stage of the attack, there were ordinarily none of those marks which are supposed to characterise, and to be necessary to prove, the existence of previous inflammation: the whole column of the alimentary canal being found paler than natural."[u] In the Madras Presidency, Scot relates that in some cases "the whole tube has had a blanched appearance both internally and externally."[x] Similar evidence is also afforded in the report on cholera in the Bombay Presidency.

---

[t] "An Essay on the Epidemic Cholera of India," second edition, 1831, pp. 41, 42.

The frequent absence of any pathological evidence of inflammation of the alimentary mucous membrane in cases of cholera has been fully confirmed by most observers of the disease in Europe, who generally agree in stating that in patients dying during collapse, there are ordinarily no signs of inflammation to be detected either in the stomach or the intestines. In Dr. Keir's very able report on cholera in Moscow, in 1830-1, the absence of inflammation, except as a secondary affection, was well established. "The inflammatory or sub-inflammatory symptoms which supervene when reaction takes place are not essential to the disease, but appear to me to be the natural effect of the congestive tendency more or less present in every case. The essential character of the disease, therefore, is not inflammation but congestion.[y] So, also, when the disease first appeared in the Austrian Empire, this absence of primary inflammation of the alimentary canal attracted much attention, and is particularly mentioned by Sophianopoulo[z], Gerardin and Gaimard, and other writers, who had the opportunity of observing the first epidemic of cholera in that country. In France, notwithstanding Broussais' assertion "that cholera is a disease eminently inflammatory,"[a] the general evidence, both during the prevalence of the first as well as subsequent epidemics of the disease, has been decidedly against the opinion that cholera is dependent on inflammation of any portion of the alimentary canal. The prevailing evidence in this country is in like manner opposed to the supposition of cholera being due to any inflammatory affection of the alimentary canal.

---

[u] "Bengal Report on Cholera," 1820, p. 82.

[x] "Madras Report on Cholera," 1824, Preface, p. 33.

The most frequent pathological condition, observed in connection with the alimentary canal of patients dying during collapse, is abdominal venous congestion. This condition has been very generally noticed in all parts of the world where cholera has prevailed, and it has been observed so commonly after death from cholera, that it must be regarded as closely connected with the disease. But even this appearance is sometimes not very decided, and it is far from being peculiar to cholera. In some cases of cholera, especially where death has occurred early, this venous congestion is not great; whilst, on the other hand, it has been observed quite as frequently in yellow and other fevers, and is very generally inferred to be present in, and connected with, the cold stage of intermittent fever, with which disease cholera has been compared by many writers; some of whom consider that the collapse of cholera is nothing more than a prolonged cold stage of ague. If such, however, were the case, it would rather increase than diminish the difficulty that has always attended any efforts to explain the occurrence of the flux; for if cholera and ague were thus closely allied, the flux which occurs in the one case should not be absent in the other.

---

[y] "A Treatise on Cholera," containing the author's experience of the epidemic known by that name, as it prevailed in the city of Moscow, in autumn, 1830, and winter, 1831; Edinburgh, 1832, p.96.

[z] The gastro-intestinal engorgement, according to Sophianopoulo, "is not inflammatory in the greater number of cases."—"Relation des Epidémies du Choléra-Morbus observée en Hongrie, Moldavie, Gallicie, et à Vienne en Autriche, dans les années, 1831 et 1832, etc., avec des notes du Professor Broussais; par le docteur Sophianopoulo," Paris, 1832, p. 124.

[a] "Le Choléra-Morbus Epidémique, observé et traité selon la Méthode Physiologique," second edition, Paris, 1832, p. 73.

Whilst there are no pathological changes apparent in the stomach or intestines, sufficiently constant or uniform, to be considered characteristic of cholera, it is very common to find in them indigestible articles of food, worms, etc.; which, under certain circumstances might be supposed capable of inducing the disease in persons already predisposed to it. In warm climates, especially in India, worms were very frequently observed to be passed by patients suffering from cholera, and also to be present in some portion of the alimentary canal after death; and this appears to have been at one time so common, as to have attracted general attention. In the first outbreak of cholera in the Presidency of Bombay, worms were so often observed in connection with the disease, as to have induced some of the surgeons to believe, that they were directly concerned in producing it. One surgeon (Mr. Milward) reported that eight patients out of ten passed worms; and from the reports of other surgeons in the Presidency, it is evident that worms were, at one time, of very constant occurrence in the cholera of Bombay. Worms are also mentioned by Dr. Keir in connection with cholera at Moscow; and they have been occasionally alluded to by other writers on cholera.[1]

Local irritation of the alimentary mucous membrane has been very generally regarded as an important exciting cause of cholera; but there are some who consider it to be not simply the exciting but the efficient cause of the disease. This theory, which appears to have been the earliest one suggested to account for the occurrence of cholera, originated with Dr. Tytler, of the Bengal Presidency; who had the opportunity of observing the disease at Jessore, where the first recognised case of cholera occurred on the 19th of August, 1817. The opinion entertained by Dr. Tytler was, that cholera was caused by diseased rice, and he, in consequence, proposed to call the disease "Morbus Oryzeus." "It is no longer matter of speculation (Dr. Tytler remarks), but of positive fact, that the *Ouse Paddy* (rice) being cut this year before it was completely ripe, and devoured by all classes of the native population, prior to its being seasoned through age, and exposure to the weather, occasioned the dreadful mortality throughout the provinces of Bengal, which, had providence not interposed, and arrested the evil by the prompt knowledge of its cause, might, in the depopulation of the country, have been productive of mischief more extended than is within the reach of human foresight to calculate, or experienced judgment to determine."[a] The opinions entertained by some writers on cholera in this country, on the first appearance of the disease, coincided with Dr. Tytler's views on the subject; but further inquiry has for the most part failed to establish their correctness, as far at least as unwholesome rice, or other grain, can be considered as the cause of the disease.

---

[1] The frequent occurrence of worms in connection with cholera in warm climates cannot be admitted as an argument much in favour of irritation of the alimentary canal being a sufficient cause of the disease; for worms have been still more generally observed in connection with dysentery and other diseases of the alimentary canal. In the following report on the health of the navy employed on the East India and China station, it is stated, that "although only nine cases of intestinal worms—the ascarus lumbricus—were entered on the sick books of the squadron, these troublesome parasites were nevertheless present in a very large proportion of the diarrhœal and dysenteric cases. They were found in all parts of the intestinal canal, and were ejected from it both upwards and downwards, sometimes naturally, at other times by medicine."—(" Statistical Reports on the Health of the Navy, for the Years 1837—1843," part ii. p. 82.

---

[a] "Remarks upon Morbus Oryzeus," Calcutta, 1820, p. 40.

With reference to unwholesome food, as the apparent cause of the flux in cholera, and according to some writers of cholera itself, I may refer to some interesting cases related by Mr. Bloxam, which are referred to by Dr. Snow, as confirming his view of cholera being communicated by direct contact of the cholera poison (?) with some portion of the alimentary canal; and which cholera poison, Dr. Snow thinks, is often conveyed by articles of diet; by their being kept at night (Dr. Snow observes) under the beds in poor people's rooms, and consequently "in close proximity with the chamber utensils."[b] The cases related by Mr. Bloxam, which are sufficiently important to quote in reference to this subject, so far, however, from confirming, appear to be directly opposed to Dr. Snow's opinion respecting the nature of cholera, and its alleged mode of communication.

"In the village of Carisbrook, which is one mile distant from Newport, there were seven cases [of cholera]. All these commenced and terminated within the period of one week; they all terminated fatally. The seven persons had all eaten of some stale cow-heels, and with one exception, it seems that they all died within forty-eight hours of the time of eating this food, and all with symptoms of cholera. After careful inquiry on the subject, it does not appear that any other persons in the village, than these seven, ate of this food, with the exception of one child, and she ate very sparingly of it. I also find that no other person in the village than these seven, had symptoms of cholera. . . . . These cow-heels had been the property of a man who died in Newport, after a short and violent attack of cholera. Two women who laid out his body, and a third who washed his clothes, died after a short illness from cholera; and it is supposed that they had eaten of the cow-heels, though the fact is not known for certain. The articles were sold at a low price after the man's death."[c]

---

[b] "On the Mode of Communication of Cholera," 2nd edit. 1854, p. 22.

These cases serve to prove that unwholesome food is a powerful exciting cause of the disease, but they do not prove more than this. The eating of unwholesome food is not unfrequent among the poorer classes, especially in warm weather, but it does not develope cholera, unless the disease be prevailing epidemically at the time. Irritation of the stomach from bad food can often be proved to have (apparently) preceded an attack of cholera. In a very large number of cases of cholera, which have come under my own observation in this country, and also in India and China, I have been able to trace the apparent origin of the attack to some improper article of diet, such as rancid bacon, tainted meat, stale vegetables, etc., of which the patient had partaken some

[c] "Prov. Med. and Surg. Journal, 1850," p. 156.

short time previously. But such sources of irritation (which have been often commented on by other writers) would act only as exciting causes in assisting to develope the disease, as a blow may be said sometimes to develope cancer; which, as far as our power of observation extends, appears to be previously present in the system in a latent form.

Under ordinary circumstances, the stomach of a healthy individual possesses the power of pouring out an increased amount of the gastric secretion, when anything of an irritating character is introduced into it. This secretion, which is acid and antiseptic in its properties, protects the system in such cases from being poisoned by bad food. When, however, cholera is prevailing, the self-protective power of the stomach is in most persons remarkably diminished; and in consequence of this, food which at other times might be taken by many people with impunity, excites in them cholera, by destroying, it would appear, the previously diminished power which the stomach possesses of protecting itself. This impaired functional condition of the stomach is well known to be very common during the prevalence of cholera, and few individuals, who escape being attacked by the disease, are altogether free from it, although it may not amount to more than a feeling of uneasiness in the stomach and bowels. This condition of the alimentary canal preceded the terrible outbreak of cholera on board the Black Sea fleet in the autumn of 1854; which outbreak of disease was, however, supposed by many to have occurred without previous warning, and was even thought to have been immediately occasioned by a "peculiar black cloud" passing over the fleet, or some other mysterious visitation. In the report of this outbreak of cholera in one of the medical journals, it is stated that "digestion and assimilation of vegetable food, were (from the early part of the summer observed to be) suspended, and the excretory functions of the liver and kidneys were diminished, or even temporarily suppressed. In most cases, the reaction of the system after this stage of depression amounted to ephemeral fever."[d]

Even unwholesome food may, under these circumstances, become a source of irritation; and excite the disease, in consequence of the stomach being not only unable to digest it, but being also incapable of protecting itself from the irritation its presence causes. In India, especially among the European residents, it is most common for cholera to occur at an early hour in the morning, generally between the hours of two and five. So much is this the case, that all medical men of experience in India, avoid giving medicines at night which would disturb the bowels early in the morning, if cholera is known to be prevalent at the time. One explanation of this is, that the body is more liable to be affected by a morbid cause during sleep, than at any other time; but this is not sufficient to account for the phenomenon; for the presence of undigested food in the stomach during sleep does not have any tendency to excite cholera, unless that disease is epidemic at the time. I would, therefore, in addition suggest, that owing to the digestive power of the stomach having been from some unknown cause diminished, the food swallowed the previous evening is not digested before sleep occurs (for the dinner hour in India is late); and in consequence of this, the individual is aroused at an early hour in the morning by cholera, which is excited by the presence of indigestible food in the stomach; and which, in the absence of an epidemic of the disease, would probably give rise to an attack of "nightmare," instead of cholera.

---

[d] "Med. Times and Gazette, 1854," p. 350.

In consequence of the frequent absence of any apparent pathological change in the stomach or intestines, sufficient to account for the flux in cholera, various theories have been proposed to explain its occurrence. One of the most plausible explanations of the flux has been thought to be that which refers it to exudation from the mucous surface of the alimentary canal, analogous to what is said to occur in certain hemorrhages. That the discharge is a "serous hemorrhage," consisting only of the fluid portions of the blood, whilst the blood corpuscles are retained within their vessels, in consequence of their size preventing escape. "I am perfectly satisfied (Dr. J. Johnson remarks) that the disease is a *serous hemorrhage* from the bowels—that is, that the serous part of the blood is drained off from the internal surface of the intestinal canal, till the powers of life are worn out, or the remaining blood becomes too thick to circulate."[e] It is difficult to understand what is meant by many writers who employ the term "serous hemorrhage," whenever there is a removal of the serous part of the blood from its vessels. It would appear that in many cases, serous hemorrhage is but another term for exudation, or what is sometimes called exosmosis. This explanation of the process by which the flux takes place, cannot be considered satisfactory; but even if it were admitted that the discharge is the result of exudation, it fails to afford any reason why the serous portion of the blood should escape from its vessels in cholera, and not in other diseases; for, even if we were to adopt Audral's views respecting the effects of mechanical hyperæmia, the supposition of the flux being the result of exudation is generally irreconcilable with the large amount and rapidity of the discharge; and it fails also to explain why the mucous membrane should be the seat of the exudation in preference to the serous membrane; for usually when a serous flux occurs from mechanical obstruction, the fluid is poured out into a serous cavity.

So great has been the difficulty in accounting for the flux in any other way, that a very large number of writers seem inclined to ascribe it to increased secretion from the alimentary mucous membrane; such a supposition being thought the only one

[e] "Diseases of Tropical Climates." 5th edit. 1836, p. 300.

capable of satisfactorily accounting for the profuseness of the flux, and the rapidity with which it is discharged. Dr. Budd, for instance, remarks that in cholera, "the abundant flux from the mucous membrane results not from mere passive congestion, but from an active process of secretion."[f] It has, however, been very generally admitted, even by those who advocate the opinion of the flux being due to secretion, that there is no evidence to prove that secretion takes place in these cases. The chemical analyses of the fluid discharged shews, that it contains only the ordinary materials of the liquor sanguinis; some of them, as the fibrine, altered it may be in physical condition, but still chemically the same in the ultimate elements. The absence also of secretion in other parts of the body, as the kidneys, lachrymal glands, etc., has been very commonly referred to, as a strong argument against the probability of the alimentary mucous membrane possessing, under these circumstances, any increased power of secretion, when this function is so manifestly arrested in more distant parts of the system. This last argument has generally been considered one of the strongest than can be urged against the theory of cholera being due to paralysis of the sympathetic nervous system; for most writers on this subject seem inclined to agree with Jameson's remark, that "the supposition of a new secretion is hardly consistent with the suspension of that action in all its established organs throughout the body."[g]

---

[f] "Medical Times and Gazette," April 16, 1853.

There are some few writers, however, on cholera, who even in the midst of the obscurity which has prevailed regarding the nature of the organic functions, seem to have had a tolerably clear idea respecting the cause of the flux. Broussais early recognised that it was probably due to a violent irritation of the mucous membrane, with excess of secretion from it; and corresponding opinions have been published by Christie, Dupuytren, Greenhow, and many other writers, some of whose names are now almost, if not altogether, forgotten. But none of them seem to have been aware of the precise nature of the process by which the flux was produced, or of the real cause of the serous part of the blood being suddenly abstracted in such large and often fatal quantities from the blood-vessels. They have for the most part simply stated, that local irritation causes increased secretion, but they have not undertaken to inform us why local and often excessive irritation does not also at other times cause a choleraic flux. The mechanism by which the flux is produced must, therefore, be acknowledged to be one of those mysteries in nature, which if any writers have tried to make out, none can be said to have succeeded in explaining.

The nearest approach to what I consider to be the correct interpretation of this symptom in cholera, was that made by the Commission appointed by the French Academy of Medicine, and whose report on cholera appeared in 1831. This report, which was stigmatised at the time as "solemn nonsense," contains in the following passage, the most profound view regarding the cause of the flux in cholera, that I am at present acquainted with: "C'est parceque les muqueuses gastro-intestinales ne sont plus animées, soutenues, vivifiées par l'influence du système nerveux, qui, frappées d'atonie, elles n'ont plus d'action réglée, par suite de l'altération de tout le système ganglionnaire; de là, les sécrétions augmentées et dépravées tout-à-la-fois dont ces membranes deviennent alors le siége."[h] It is hardly in my power to add to or substract anything from this passage, which can render it more complete. I believe it to contain a correct statement of the circumstances under which cholera is developed; it at once explains the nature of the disease, and justly ascribes the flux to paralysis of the sympathetic nervous system; and although the mechanism by which the flux is produced may not appear to be very distinctly referred to, it is remarkable that this important report on cholera should, at the time, have been read only to be ridiculed, and ridiculed only to be forgotten.

---

[g] "Bengal Report on Cholera, 1820," p. 84.

It is to me evident, that the only satisfactory explanation that can be offered to account for the flux in cholera, is that which ascribes the disease to paralysis of the central portions of the sympathetic nervous system, in consequence of which the secretory function of the stomach is increased, whilst the fluid eliminated by the mucous membrane is no longer the proper gastric secretion, but the nutritive part of the blood. This, as will be subsequently explained in describing the nature of the organic functions, is a necessary consequence of paralysis of the sympathetic nervous system. It must, therefore, be inferred, that the central portion of the sympathetic nervous system is paralysed in cholera, because the flux from the stomach and bowels consists of the nutritive part of the blood; and that such is the source of the flux is confirmed by numerous analyses of the blood itself, which prove that the blood has lost those essential elements, which are found in the flux from the alimentary canal. It is on this account that I consider it so desirable to ascertain the cause of the alkalinity of the flux in cholera; for although I know theoretically that it must be due to the presence of soluble phosphates, especially the phosphate of soda, yet I am aware that the argument would necessarily appear to many more satisfactory, if it were more strongly supported by chemical analysis. In all cases where the ordinary secretion from a mucous membrane is replaced by one containing the soluble phosphates in excess, there is paralysis or disturbance of the sympathetic nervous system: and the principal soluble phosphate present in these cases appears to be the phosphate of soda, which according to Liebig "is indispensable to the normal constitution of the blood."[l]

---

[h] "Rapport de l'Académie Royale de Médicine sur le Choléra-Morbus," Paris, 1831, p. 42.

Some of the relations between the excretion of phosphates, and diseases of the nervous system seem to have been carefully studied by Dr. Bence Jones,[k] who has endeavoured to shew, "that acute affections of the nervous substance organic and functional, are the only diseases in which an excess of earthy and alkaline phosphates appear in the urine." The source of the phosphates in these cases, is supposed by Dr. Jones to be due to the oxydation of nervous substance; and as he found that the amount of phosphates was sometimes excessively diminished in delirium tremens, he seems inclined to think, "that the action of oxygen on the nervous substance is lessened by alcohol, and that by acute inflammation, or excessive excitement of the brain, it is preeminently increased."

In acknowledging the great value of Dr. Jones' chemical researches, I cannot admit that I agree with his explanation respecting the origin of the phosphates; and I think the fact he has recorded, that mental excitement produced by alcohol diminishes the amount of phosphates in the urine, whilst excessive mental excitement from other causes

---

[l] "Researches on the Chemistry of Food," edited by Gregory, 1847, p. 112.

[k] "Contributions to the Chemistry of the Urine," Phil. Trans., 1845: and, "Papers on Chemical Pathology" (Lancet), 1847.

"pre-eminently" increases it, is sufficient to shew, that when phosphates in excess occur in the urine, they do not necessarily represent a corresponding portion of oxydised brain. The explanation Dr. Jones has offered respecting the source of the phosphates in these cases, not only appears to be opposed to the results of his analyses, but is even contradicted by the deductions which he makes from his own observations, as in the following passage: "That there exist two kinds of alkalescence of the urine; the one long known as ammoniacal, the other not distinctly recognised, arising from fixed alkali. This last appears most frequently in water secreted from two to four hours after breakfast, in persons suffering only from indigestion."[1]

Some other explanation seems, therefore, to be required; and without entering into any discussion respecting the influence which the brain can often be shewn to possess over the secretion of urine, I would state, that the only satisfactory way of explaining this difficulty appears to be that of supposing the brain, in these cases, to act only through the medium of the sympathetic nervous system; and that the excess of phosphates in acute disorders of the brain, is the result of the impression communicated from the brain to the sympathetic nervous system, and which so far interferes with its influence over the secretion by the kidneys, as to allow them to eliminate an excess of phosphates from the blood, instead of (or in addition to?) the ordinary elements of the urine. If the explanation I shall subsequently offer respecting the functions of the sympathetic nerve be correct, it will be evident that not only should we expect to find this excess of phosphates in the urine, as a consequence of acute diseases of the brain; but that in all cases of alcoholic poisoning, when the affection of the brain is secondary to that of the blood, we should theoretically infer that the phosphates would either be absent from, or greatly diminished in, the urine.

---

[1] "Phil. Trans.," 1845, p. 348. With reference to the cause of the alkaline reaction of the urine in these cases, Dr. Jones remarks, that "in the one case the alkalescence arises from volatile alkali, and in the other from fixed alkali. In the first, it is caused by carbonate of ammonia, and in the second, by carbonate of soda, or potash, or alkaline phosphate of soda. Decomposition of urea is the origin of the one, and disordered secretion of the other" (p. 343).

It will be perceived, from the preceding remarks, that it is necessary that the morbid cause, whatever may be its nature, should be applied directly to the surface of the mucous membrane, in order to produce a perverted secretion from the part. For although it will be invariably found, that wherever the ordinary secretion from any of the mucous membranes is increased and perverted, that there is an undue or excessive elimination of the alkaline phosphates; still the affection of the sympathetic nervous system, leading to an increased and perverted secretion from the mucous membrane of the part, may be produced in two ways: either by direct irritation of the mucous membrane referred to, or by an impression communicated to it from the cerebro-spinal system; the impression so communicated being due to an acute disorder, either functional or organic, of the brain or spinal marrow. To which of these causes the paralysis of the sympathetic nervous system in cholera is to be ascribed, I am unable at this stage of the inquiry to say. It is possible, that both local irritation of the alimentary mucous membrane, and an impression from the cerebro-spinal system, may concur in producing that central paralysis of the sympathetic nervous system which manifests itself in the characteristic flux of cholera. But until the function of the sympathetic nerve has been fully explained, it will be impossible to describe clearly the mechanism by which the alkaline flux in these cases is produced.

(*c.*)—*Its relation to collapse.*

In consequence of the doubt which has prevailed respecting the nature of cholera, it has been found difficult to account satisfactorily for either the origin or the extent of the collapse. A large number of those who reject the theory of cholera being due to an affection of the sympathetic nervous system, have endeavoured to show that the collapse is simply the result of the drain from the blood; and that its intensity is always in proportion to the amount of the fluid lost. This was an opinion very early entertained on the subject in India; and although further experience in the disease soon convinced most of the Indian practitioners that it was impossible so to account for it, but that some other cause must also be concerned in its production, it has since been repeatedly put forward as a sufficient explanation, and is considered as such by many writers on cholera of the present day; among the most recent of whom is Dr. Snow, who observes, "In all cases of cholera that I have attended, the loss of fluid from the stomach and bowels has been sufficient to account for the collapse, when the previous condition of the patient was taken into account, together with the suddenness of the loss, and the circumstance that the process of absorption appears to be suspended."[m]

There are, however, many who have had great experience of the disease in this country, and who acknowledge that they have not unfrequently met with cases in which this explanation of the cause of the collapse was insufficient. Dr. Gull observes, "that although, in a large number of instances, the intensity of the symptoms is in a general way proportionate to the amount of the effusion, yet that this will not in part explain the attendant collapse which often appears to be in no inconsiderable degree due to the adynamic state of the ganglionic nervous system."[n]

Dr. Johnson, also, who may be said to have revived the eliminative treatment of cholera, goes a step further in stating, "that there is no relation between the degree of collapse and the amount of fluid which is lost by purging; that in many cases there is rather an inverse ratio between the collapse and the diarrhœa; and that the former often decreases and disappears, while the latter continues with unabated rapidity."° Whatever opinions may be entertained respecting the merits of Dr. Johnson's mode of treatment, which is based chiefly—but, as I am inclined to think, very erroneously — on this observation respecting collapse, the truthfulness of his evidence cannot be gainsaid, as it is derived from close study of cholera, and accords, moreover, with the experience of others, whose opportunities of watching the progress of the disease during successive epidemics of it in this country have been considerable.

---

[m] "On the Mode of Communication of Cholera." 2nd edit., p. 11.

[n] "Report on the Morbid Anatomy, Pathology, and Treatment of Epidemic Cholera." 1854, p. 134.

It is not, however, by examining the evidence derived from cases of cholera occurring only in this country that this point can be satisfactorily determined, but by taking a wider range, and inquiring into the particulars of the disease as it manifests itself elsewhere. The histories of the first epidemic of cholera in India contain frequent allusions to cases in which the disease proved quickly fatal, before any vomiting or purging had occurred. During the outbreak of cholera in Bengal, in 1817 and the following years, it was observed, that "in some rare instances the virulence of the disease was so powerful

---

° "Med. Times and Gaz.," Sept. 9, 1854.

as to prove immediately destructive to life; as if *the circulation were at once arrested*, and the vital powers wholly overwhelmed. In these cases, the patients fell down, as if struck by lightning, and instantly expired. Others, again, sunk after making one or two feeble efforts to vomit, and drawing a long and anxious inspiration. Some recovered from the insensibility produced by the first shock, and afterwards went through the regular course of the disease."[p] Similar cases were also observed in the other presidencies of India; and this supposed variety of the disease was distinguished at the time by the name of "*Cholera Sicca.*" The examination, however, after death, of the stomach and intestines, satisfied medical observers that the name was erroneous, and that the usual flux, deficient only in quantity, had occurred; but, at the same time, it taught them that death was the result of something more than "a serous hemorrhage," and consequently it was very early assumed to be due to an affection of the nervous system.

The general want of uniformity in the amount of fluid lost and the subsequent collapse, was even more noticeable in the outbreak of the disease in Russia than it had been in India. From the official report by Drs. Russell and Barry, already alluded to, it is evident that the collapse was to a great extent independent of the discharge from the alimentary mucous membrane; and it was owing to this that, in Russia, the disease was very generally supposed to be the result of some affection of the sympathetic nervous system. The extent to which this opinion was adopted, and at one time prevailed throughout Europe, and the consequent influence it had on the treatment of disease, is abundantly shewn by the contemporary medical literature of this and other countries.

---

[p] "Bengal Report on Cholera," by Jameson. Calcutta, 1820, pp. 41, 42.

Each succeeding epidemic of cholera has afforded examples of this fatal form of collapse, without any premonitory flux from the stomach or bowels. One of the most recent and remarkable instances on record of this sudden invasion of the disease occurred in the Black Sea fleet, off Baljik, in the autumn of 1854. "The first fatal case that occurred in the British fleet," Sir William Burnett relates, "took place in the 'Sanspareil,' July 31st, 1854. The man declared that he had been in perfect health up to the moment of his seizure. About four o'clock in the morning he was roused out of his sleep by severe cramps and other unmistakeable symptoms of cholera, and, in spite of a variety of remedial measures, he died about six o'clock in the evening." The disease on this occasion seems to have had a peculiarly malignant character; for, as Sir William Burnett proceeds to relate, "the attacks in many instances were so sudden, that many men fell as if they had drunk the concentrated poison of the Upas tree."[q]

---

[q] "Report on the Cholera which attacked the Fleet in the Black Sea in August, 1854."

Besides the evidence derived from actual observation of the disease, it might be inferred from analogy that the collapse of cholera is not necessarily dependent on the flux from the stomach and bowels, as the same form of collapse occurs in other disorders affecting the stomach without any such discharge. The symptoms accompanying perforating ulcers of the stomach are so nearly identical with those of cholera, with usually this one exception of the absence of a serous flux, that they may be readily mistaken, as I have already pointed out, for those of cholera.

Inverse evidence to the same effect is afforded in cases of so-called choleraic diarrhœa, in which the peculiar discharge of cholera may be observed to continue for a considerable time, and even to become somewhat profuse, without giving rise to any corresponding symptoms of collapse. If a small amount of the flux is often sufficient in cholera to produce collapse, it may consequently be asked how it happens that, in cases of choleraic diarrhœa, pints of this peculiar fluid are discharged without a severe amount of collapse being developed? As the flux in both cases is apparently derived from the same source, and is analogous in composition, why should it induce fatal collapse in the one case and not in the other? The fact of the flux in choleraic diarrhœa being often profuse without giving rise to collapse, may be said to afford a double proof respecting the nature of cholera: 1st, that the characteristic flux of cholera proceeds chiefly from the intestinal canal, since the same kind of flux occurs in choleraic diarrhœa, without the stomach being involved in the mischief; this is proved by the stomach, under these circumstances, retaining its absorbing power: and, 2nd, that as this flux may continue for a considerable time without developing collapse, it may be inferred, that what is commonly recognised as cholera is a something added to choleraic diarrhœa, and which has a necessary tendency to develope collapse; and as the stomach has been observed to lose its absorbing power in proportion as the collapse is developed, the cause of the collapse must be intimately connected with the morbid affection of the stomach.

In rejecting, however, the hypothesis of the collapse of cholera being caused by the flux from the alimentary mucous membrane, I still consider that there is often a close and important relation between them; and that, although the flux does not originate, it may intensify and prolong the collapse, by diminishing the fluidity of the blood, and consequently impeding its circulation.

The origin of the collapse in cholera I believe to be due to a local diminution or arrest of power in the vascular system, in consequence of which the balance of the circulation is disturbed; and as the circulation in the extreme parts of the system fails in many cases independent of, and often previous to, the occurrence of any flux from the alimentary canal, it may be inferred, that the blood might be quoted in confirmation of these remarks, which, at one time, appear to have hardly admitted of dispute. But of late years, it has become somewhat the custom to speak of cholera as a peculiarly insidious disease, always preceded by diarrhœa, and that this premonitory diarrhœa passes by easy transition into cholera. So far, however, judging from my own experience, is this from being the case, that in most of the cholera patients who have come under my observation, there appears to have been a sudden invasion of the disease; and the patient has generally been able to refer to a particular period in the history of the attack, when after suffering, it may be, for some time from diarrhœa, a rapid change for the worse has marked the accession of cholera.

As the disease advances, the failure of the circulation becomes more complete, and the extremities, in cases attended with very severe collapse, often appear to be completely drained of blood; for not only are the edges of the incision in venæsection, in many cases, perfectly bloodless, but the brachial artery, on being cut into during life, has been found empty and contracted.

Whilst the circulation in distant arteries thus fails, the pulse sometimes continues to be discernible in some of the larger arteries near the trunk, as in the carotids, and the circulation does not appear to fail in these vessels till just previous to death. The supply of blood to the brain, throughout the whole course of the disease, seems to be sufficient for the continuance of the mental functions, except in those occasional instances in which the suddenness and intensity of the attack induce syncope; but, even in these cases, the patient is generally observed to rally quickly, and the mind subsequently remains unaffected until the last moments of life.

The uninterrupted and apparently undisturbed flow of blood through the brain, at a time when the circulation in other parts of the body is either greatly diminished or altogether arrested, is one of those problems in cholera, which it would be difficult, and perhaps, in the present state of our knowledge on the subject, impossible to solve. It is evident that the blood-vessels within the cranium are, to a great extent, independent of the influence which regulates the condition of the blood-vessels elsewhere, for otherwise the brain could not long remain unaffected; and it is equally evident that the blood in them must be more fitted to circulate, than it is in those vessels in which its increased consistence offers a physical impediment to its progress. This apparent independence of the vascular system in different parts of the body may hereafter admit of being satisfactorily explained, by referring it to the restricted influence of its nerves; which being (for the most part) derived from the sympathetic, probably do not, except in extreme cases, transmit impressions beyond the nearest ganglia. But as it is hardly allowable in the present instance to do more than offer this suggestion, I would only add, that whilst the state of the intra-cranial circulation in cholera is consistent with functional disorder of the abdominal portion of the sympathetic nerve, it cannot be made to harmonize with any other theory of the disease that has been proposed. It might even be assumed that as the cerebral arteries differ in their structure from the arteries elsewhere, in being altogether deficient in one of the coats (the muscular?), that the nervous influence they receive may differ from that supplied to the other arteries of the body. If such were the case, the force which contributes to the circulation of the blood through the brain, would not only differ from that which effects it in other parts of the system, but the intra-cranial circulation would be for the most part independent of such causes, as disturb the circulation in those organs appropriated to nutrition. It is important to notice, that the maintenance of the circulation of blood through the brain in cholera, corresponds with what has been observed in cases of perforating ulcer of the stomach, etc.; which, from the collapse in the two cases being so remarkably alike, affords strong presumptive evidence in favour of the supposition that the seat of the disease may, in each case, be referred to the same part of the system.

The general failure of the circulation in cholera has been referred, by different writers, to an altered condition either of the blood or its vessels. The first of these opinions seems to be that which has been most commonly entertained, and is the one necessarily adopted by all who consider cholera to be the result of a specific poison in the blood; but the arguments on which it rests appear to be insufficient to to prove that it is the primary cause of this phenomenon. The great change which takes place in the physical condition of the blood, in consequence of which it often subsequently becomes almost too thick to circulate, is dependent on the amount of the peculiar flux from the stomach and bowels; and consequently, the pulse at the wrist, according to this view, should not fail until the fluidity of the blood has been considerably diminished. But instead of this being the case, it has been a subject of common observation, since the first appearance of the disease in Bengal, that the pulse at the wrist has often ceased for some time previous to the blood having suffered any considerable apparent change; and that not unfrequently, after a patient has remained pulseless for some hours, and then died, the blood has been found to retain sufficient fluidity to have allowed of its circulating freely during life. As therefore the circulation may, and frequently does, fail before any physical alteration has occurred in the condition of the blood, adequate to cause much impediment to its movement in the vessels, it follows, that if the primary cause of the failure is due to the blood itself, it can only be ascribed to some change affecting its so-called vital properties. With reference to this point, it may be at once allowed, that there is abundant evidence to prove, that during the collapse of cholera, many of the normal changes connected with the blood either cease altogether, or are greatly diminished; such as the development of urea, and other compounds, which, during health, are constantly being added to and removed from the blood. But even in the present state of our knowledge on these subjects, it is probable that the formation of such compounds is as much due to the condition of the blood-vessels, as of the blood itself; and that the force necessary for their development, not only cannot act on the blood independent of its vessels, but is in all cases transmitted by their parietes. Consequently, the changes which occur in the blood during life cannot, correctly speaking, be said to originate in the blood itself; but to be dependent on the condition of its containing vessels, and effected only by their agency.

This opinion is supported by the fact that, in the deeply collapsed stage of cholera, the same effects have been observed to follow the employment of galvanism as of saline injections into the veins. Both these remedies have frequently been found to possess remarkable revivifying influence on the patient, and to succeed in completely rousing him for a time from a state of intense collapse, so as to be able to sit up and talk even cheerfully. The pulse, under these circumstances, returns to the wrists; and with the circulation the organic functions are often temporarily restored. With reference to the explanation of these phenomena, it has generally been alleged that in the case of saline injections, the condition of the blood, after being deprived to a great extent of its fluidity, is such as to offer a physical impediment to its onward progress; and that by restoring the blood to its fluid state the obstacle is removed, and the circulation is in consequence restored. Although this may seem to be a very reasonable explanation of the fact, I cannot suppose that it is simply by restoring the lost fluidity of the blood, that the circulation is re-established in these cases; for, if such were the case, the circulation should continue to be maintained so long as the blood was kept sufficiently fluid. Yet so far is this from being usually observed to follow the use of saline injections into the veins, that there appears to be, after a time, a decided tendency to increase of collapse, which the repetition of saline injections cannot overcome; so that the circulation ultimately fails, independent of the physical condition of the blood.

This is illustrated by the effects of galvanism on the circulation, which produces the same results as saline injections into the veins; except that, of the two, galvanism is more permanent in its effects, and attended with more satisfactory results. The restoration of the circulation in these two experiments, may appear at first sight to be effected by very opposite means; but in reality there is not much difference between them. In the one case, that of saline injections, the diminished power of the circulating organs to carry on the circulation, is remedied for a time by diminishing the obstruction to be overcome, which obstruction is caused by the crassitude of the blood to be moved; and the circulation is also in some measure assisted by the stimulating effects of the saline injections on the parietes of the vessels. In the other case, that of galvanism, the obstruction to be overcome continues, but the force applied to overcome it is increased.

The circulating system in cholera may in this respect be compared to a heavily laden waggon, in which the weight to be moved is greater than the power of the horses to move it. The difficulty admits of being overcome in two ways, either by partly unloading the waggon, and so diminishing the weight to be moved; or by attaching an extra horse to the waggon, and so increasing the motive power. By either means, a corresponding effect can be produced, and the requisite speed can be attained.

In rejecting, therefore, the theory that the failure of the circulation in cholera is primarily due to the blood, it remains to be considered in what way an altered condition of the blood-vessels can be instrumental in producing it. This it will be my endeavour to shew may be effected through the medium of the sympathetic system, which supplies the nerves distributed to the blood-vessels, and which appears to possess an important control over their condition.[u]

---

[u] "The normal contraction of arteries is probably excited through the instrumentality of the nerve-fibres of the sympathetic system distributed to their walls, and connected through the medium of

The principal use of the sympathetic nerve, is apparently to regulate the distribution of the blood, and to prevent an excessive accumulation of it in particular organs, during an increased and temporary activity of their functions. This theory coincides, to a certain extent, with the opinion now very generally entertained, that nutrition and secretion in animal bodies can take place independent of nervous influence, an opinion which is founded chiefly on the supposed absence of anything analogous to nerves or nervous structure in plants. But as the nutrition of man and the higher animals differs from that of plants, in having special internal organs for the reception of food, and for the separation of waste materials, so is there a necessity in them, which does not exist in plants, for a special controlling power, capable of regulating the distribution of that fluid by which their nutrition is effected: and of preventing its excessive accumulation in particular organs during the activity of their functions. For such local accumulation of blood would almost (?) necessarily occur, if the circulation in more distant parts of the system, were not in some measure independent of such powerful and periodic disturbing causes, as the introduction of food, and the changes consequent on its assimilation.

The power of maintaining the circulation of the

---

ganglia with the fibres supplying the organ to which such arteries convey blood." ("Handbook of Physiology," by Kirkes and Paget, 1848, p. 111.)

blood in one part of the body independent of the rest, the sympathetic may be said to possess in virtue of its ganglia, which serve as barriers to limit the transmission of impressions, and to guard against the injurious effects which would otherwise accrue to the system at large, from the constant occurrence of local disturbing causes.

The structure and distribution of the sympathetic nerve are conformable to this explanation of its use; and, although it is hardly allowable to suppose, that one part of the system more than the rest exhibits any unusual adaptation of structure to function, since this everywhere prevails, but is not perhaps equally manifest to us, I cannot forbear alluding to this point as a strong argument in favour of the theory proposed. As the stomach, from its central position and its use, is the most important of the organs appropriated to the nutrition of the body, it is the more necessary to defend the system from the great and frequently recurring disturbance occurring in it, consequent on the introduction of food, and its conversion into chyme, than from any changes which may occur in other organs, which, compared with the stomach, are only entitled to be considered as the branch establishments of nutrition. In accordance, therefore, with this view, it might be inferred, that the most important barrier against the transmission of organic impressions, would be provided in the immediate neighbourhood of the stomach itself. Consequently the size and position of the great semilunar ganglia may be referred to as affording anatomical evidence in favour of this theory.

In like manner, the relative size, number, and position of the different ganglia of the sympathetic in other parts of the body, seem to be regulated by the nature and relative importance of the organic functions over which they preside; and so effectually do the ganglia appear to isolate the organs which they may be said to guard, that the ordinary impressions consequent on increased activity in one part of the system, do not seem to be capable of disturbing the healthy balance of the functions.

With reference to the stomach, which as the receptacle for food is more liable than other organs to great and sudden changes, safety is insured by means of the great solar plexus with its ganglia, which from their size and position effectually protect the system from being injuriously affected during digestion. As the danger in this case is great from the suddenness of the change, caused by the introduction of food, so it is the more necessary to oppose an effectual barrier to its transmission. But in the lower parts of the alimentary canal, where the change is less sudden, the same object is obtained with smaller ganglia; but which from their distribution appear to be admirably adapted for the duties they have to perform. The vertebral chain of ganglia may be regarded as the last or innermost of these defences, and from their position as destined to protect the cerebro-spinal system from injurious organic impressions; and which, if they do not altogether arrest, they probably contribute to modify.

But whilst the development of the sympathetic ganglia in each part of the body is thus proportionate to the probable amount of disturbance, and sufficient to guard the system from the ordinary impressions consequent on healthy nutrition; the ganglia are insufficient to prevent the transmission of unusually sudden and intense impressions; which consequently act injuriously on the system, and may even prove quickly fatal. A violent and unexpected blow upon the epigastrium, or the incautious drinking of cold water when the body is heated, may suddenly destroy life, in consequence of the semilunar and other ganglia being ineffectual to prevent the impression on the stomach from being at once communicated to the rest of the system. The converse of this occasionally happens in death from mental emotion, by a sudden and intense impression from the brain being instantly communicated to the sympathetic nervous system, and causing fatal, because prolonged, arrest of the circulation in the organs of nutrition. The commonly received statement, that death from mental emotion is caused by immediate arrest of the heart's action, incompletely expresses the effect produced in these cases: for the arrest of the heart's action does not precede, but is either simultaneous with, or the result of, an arrest of the circulation in the abdominal organs. This may be illustrated by what occurs in those cases where an opportunity sometimes presents itself of judging of its effects on those organs, over which the sympathetic more exclusively presides. The occasionally unpleasant consequences of fear on the alimentary canal, may be cited as a familiar instance of the extent and nature of an emotional impression suddenly transmitted from the brain; and this is further corroborated by the coldness and pallor of the surface, which amount sometimes to temporary collapse.

In this respect, the ganglia of the sympathetic may be said to serve partly the same purpose as the buffers on a railway carriage, in so far arresting or modifying ordinary impressions, as to ward off injury to the system generally; but like them, also, they appear to be equally inefficient in preventing the transmission of violent and unexpected impressions which destroy life, as a violent collision may destroy a train of railway carriages, by the shock being communicated suddenly, and with scarcely diminished force, to the rest of the system.

The superficial and restricted effects of many emotional impressions are not opposed to this view of the functions of the sympathetic, but tend rather to confirm it. For in the case just referred to, where the sudden emotion of fear is represented as causing an arrest of the circulation of blood in the abdominal viscera, no determinate direction having been given to the impression, it necessarily affects the nutritive organs, and especially the alimentary canal, in consequence of the extent and functional activity of its secreting surface, and from its being more exclusively under the control of the sympathetic nervous system. For reaction, or the effort to recover from the effects of the impression, must be slower and more uncertain where the alimentary canal is concerned, than it would be in other parts of the system, which are either of less importance, or more independent of the sympathetic nervous influence. As the barrier to be overcome before an impression from within can reach the alimentary canal is great, so must there be a corresponding difficulty in recovering from the injurious effects of the impression when it has once produced its effects on an organ so well protected. But in those cases in which the emotional impression receives a definite direction, the effects of the impression may be manifestly limited to the part to which the impression is determined. The phenomena of blushing may be referred to as an illustration of this. The colour and heat which are suddenly developed in this case are caused by a dilatation or relaxation of the blood-vessels in the part, consequent on a local and temporary arrest of their nervous influence. The suddenness with which the effect is produced, contrasted with its gradual removal, naturally suggests the inference, that when the circulation in the most important of the abdominal viscera is in like manner affected, the recovery is proportionately slower and more difficult. The increased development of heat, which always accompanies these and other local arrests of the circulation, is a fact which may be made use of in explaining the cause of the local (especially the epigastric) development of heat in cholera.

In these cases where limited arrests of this circulation have been caused by determinate emotional impressions, the condition of the blood-vessel is apparent, and the increased development of heat is a necessary consequence of the increased amount of blood in the part. But these illustrations might be usefully carried much further, and additional phenomena will be witnessed if the local effect of a determinate mental emotion be manifested on a mucous surface instead of the skin. When the display of food to a hungry man is said to "make his mouth water," the effect produced is analogous to that which occurs in blushing, with this difference: that the emotional impression, or desire to taste food, being determined to a surface lined with mucous membrane, an increased secretion from the parts occurs, in consequence of the additional supply of blood, which enables, or rather induces, the parts to carry on more actively than usual their secretory function.

The effect produced in this instance is, in a milder degree, similar to that which takes place in cholera; for however complicated some of the phenomena of cholera collapse may appear, they are all dependent on the condition of the blood-vessels in the central organ of nutrition; and so long as the nervous influence which should be supplied to the blood-vessels in this part of the system is withheld, so long will the collapse continue, until at last the other vital functions cease in consequence of not receiving the necessary supply of blood; and death closes the scene. The alleged contradictory appearances presented by the alimentary mucous membrane, after death, in various periods of the disease, are perfectly consistent with this view of its nature; and as I shall endeavour to prove, when describing the nature of the organic functions, they are no more than what we should theoretically expect to meet with in this disease.

The anatomical evidence derived from the distribution of the sympathetic nerve and its ganglia in the lower animals is so far in favour of this theory, that I may safely appeal to it as furnishing an argument, quite as strong as is usually derived from such a source, in favour of the alleged function of any other part of the system. But as nearly all evidence of this nature is to a certain extent fallacious, from its apparently admitting of being differently interpreted, I feel that it will be necessary to have recourse to other sources of information, to corroborate the evidence already adduced, and to prove the correctness of its application.

With this object, I would now direct attention to the effects which have been observed to follow division of the sympathetic nerve in the neck, as affording experimental proof of the connection between the sympathetic nervous system and the circulation of the blood. In these experiments, which have been performed by MM. Pourfour de Petit, Biffi, Rueti, Claude Bernard, and Budge, Dr. Handfield Jones, and others, some curious and important facts have been elicited connected with the physiology of the sympathetic nerve, which may be applied to elucidate some of the phenomena connected with the circulation in cholera.

M. Pourfour de Petit, whose first experiment of section of the sympathetic (*l'intercostal*) in the neck was performed at Namur in 1712, and who subsequently confirmed his observations by several similar experiments, found that division of the sympathetic (with the eighth nerve) opposite the third or fourth cervical vertebra, had the effect of impeding the circulation in the superficial vessels of the eye on the same side, and of increasing the amount of lacrymal fluid discharged, owing, as he states, to relaxation of the parts; and that the loss of this fluid rendered the blood in these vessels more consistent. "Le relâchement," M. Petit observes, "de ces parties est si évident, qu'il arrive presque toûjours une légère inflammation dans la conjonctive, par le gonflement de ces vaisseaux; mais pendant que ces vaisseaux se gonflent de sang à l'extérieur de l'œil, et qu'ils fournissent une grande quantité de liqueur, l'épaississement que ce même sang acquiert dans ces vaisseaux relâchés, et qui ont la liberté de se dilater, l'empêche de pénétrer avec facilité dans l'intérieur de l'œil où les vaisseaux sont très pressés et resserrés par la sclérotique qui a un fort grand ressort, et par les autres membranes de l'œil. Ce sang y fournit moins d'humeur aqueuse, ce qui produit l'affaissement de la cornée et le moins de brillant que l'on remarque à l'œil."[x]

These results have been very generally confirmed by many subsequent inquirers, whose observations, however, like those of M. Pourfour de Petit, have been restricted to the effects produced on the eye. But within the last few years, M. Claude Bernard[y] has directed attention to the consequences of section of the sympathetic in the neck, on all those parts to which the cut portion of the nerve is distributed; and he has succeeded, by an extended series of experiments, in greatly increasing our information on this hitherto obscure subject. In the year 1851, M. Claude Bernard, at a meeting of the Société de Biologie in Paris, demonstrated that one of the effects of section of the sympathetic was to increase the quantity of blood in all the parts supplied with the nerve, and at the same time to increase their temperature. Dr. Handfield Jones[z] has lately repeated the experiment of section of the sympathetic in a cat, and has observed similar hyperæmia, with increased temperature of the parts, to occur; and for some days, he states, considerable conjunctivitis, with copious muco-purulent discharge.

---

[x] "Histoire de l'Académie Royal des Sciénces," Année 1727: Paris: p. 14.

[y] "Comptes Rendus et Mémoires de la Société de Biologie:" Paris, 1851—1855.

[z] "Lancet," 1855, p. 47.

That the hyperæmia in these cases is a direct result of section of the sympathetic admits of being proved by galvanism; for on applying the galvanic wire to the upper cut end of the nerve the hyperæmia disappears, but, on discontinuing the application of galvanism, it returns. From the researches of M. Claude Bernard it would appear, that however often this experiment is repeated, the results are always the same, provided the animal be strong; and it has been ascertained that a hyperæmic, or a converse condition of the parts, may, after section of the sympathetic, be produced at pleasure by means of galvanism.[a]

There is considerable difference of opinion among experimental inquirers in this branch of physiology, respecting the nature of the influence which the sympathetic possesses over the circulation of the blood, and whether the hyperæmia following section of the nerve is the result of increased action or paralysis. M. Claude Bernard thinks that the congestion in these cases is caused by increased vascular action; and he refers to the forcible beating of the arteries in the part as a sufficient argument against its being the result of paralysis. Although M. Bernard's opinion on these matters must be allowed to possess great weight, I should be more inclined to adopt the explanation originally suggested by M. Pourfour de Petit, that on cutting off the sympathetic nervous influence, the vessels become relaxed, and in consequence distended with blood; unless, as M. Petit remarks, their dilatation be prevented by the unyielding character of the surrounding structures. This view accords with the influence which galvanism has been shown to possess in overcoming the vascular congestion, which it can only be supposed to effect by restoring for a time the contractile power of the arteries; and it therefore seems to me to be the most reasonable explanation of the phenomenon. Dr. Handfield Jones appears to be also inclined to adopt this as "the most probable opinion."

---

[a] " Le galvanisme produit les effets diamétralement opposés. Si on galvanise le bout supérieur du grand sympathique divisé, tous les phénomènes qui ont dû se produire changent de face: . . . . d'action qu'elle était, la circulation devient faible; la conjonctive, les narines, les oreilles, qui étaient rouges, pâlissent.

" On cesse la galvanisation. Tous les phénomènes primitivement produit par la section reparaissent peu à peu, pour disparaître de nouveau à une seconde application de galvanisme. On peut continuer à volonté cette expérience, la répéter autant de fois que l'on voudra, toujours ses résultats seront les mêmes; la seule condition, c'est d'agir sur des animaux vigoureux, tels que des chevaux et des chiens."—" Sur les Effets de la Section de la Portion Céphalique du grand Sympathique:" par M. Claude Bernard. " Comptes rendus des Séances de la Société de Biologie," 1852, pp. 169, 170.

The preceding remarks have been directed to show, that the anatomical and the experimental evidence both tend to confirm the theory, which refers the failure of the circulation in cholera to a central arrest of the sympathetic nervous influence. But there remains to be considered the last, and probably the most conclusive, evidence that can be adduced in its favour; *viz.*, its relation to the physiological evidence respecting the forces which move the blood.

In attempting to describe the causes which probably conduce in cholera to the failure of the circulation, it is necessary to have a clear understanding of the forces which are employed to circulate the blood in health. It has long been recognised by some physiologists, although it is still denied by many, that the heart is not the only propelling power: for even if it could be allowed to be sufficient for that purpose in man and the higher animals, it is necessary to have recourse to some other force in many of the lower animals, who are not provided with a heart, or any contractile organ that can be supposed to serve a similar purpose, and in whom, consequently, the circulation must be carried on by some other motive power. For an ingenious explanation of the nature of this (supplemental) force in man and the higher animals, and its mode of action in all organised bodies which possess a circulation, we are chiefly indebted to Dr. Draper[b] of New York, who has succeeded in partly establishing the fact, that the same force which, in plants, causes the movement of the sap, effects also the movement of the blood in animals.

"The cause of the movement of the sap," Dr. Draper observes, "in flowering plants, both of the rise of the crude sap upwards, and of the descent of the elaborated sap downwards, is the light of the sun, which effects the decomposition of carbonic acid gas."

---

[b] "A Treatise on the Forces which Produce the Organisation of Plants, with an Appendix, etc." 2nd edit., New York, 1845.

The motion which takes place in this case is said to be the result of capillary attraction; and the same principle has been applied to account for the circulation of the blood, which, according to Dr. Draper, depends on the following law: "If two liquids communicate with one another in a capillary tube, or in a porous or parenchymatous structure, and have for that tube or structure different chemical affinities, movement will ensue; that liquid which has the most energetic affinity will move with the greatest velocity, and may even drive the other fluid entirely before it."

In the application of this law to the circulation of the blood, it is scarcely necessary to observe, that the force employed must be always the same, however much the circumstances may differ which lead to its development. Thus "the oxygenizing action of the arterial blood is the true cause, according to Dr. Draper, of the systemic circulation."

In the pulmonary circulation, the converse of this is said to occur. The affinity is here shewn to exist between the venous blood and the oxygen of the air, in consequence of which the venous blood is attracted to the pulmonary capillaries, where carbonic acid and oxygen mutually replace each other. The oxygenated blood being urged onwards by the attraction of the venous blood for the oxygen of the air, constantly pressing it forwards. Hence it might be inferred, that in cholera we should not meet with pulmonary congestion, unless the lungs had been affected similarly to the alimentary canal, or the formation of carbonic acid in the system had become excessive.

According to Dr. Draper, there "are three sources of force engaged, under normal circumstances, in directing the portal circulation. One of these is found in the aortic capillaries, when they are spread on the chylopoietic viscera, the modification being precisely analogous to that which obtains in the general systemic circulation. The other two are found in the liver itself; the first is a pressure exerted by the portal blood on that of the hepatic veinlets; the second, by the blood of the hepatic artery, which, conspiring with the former, urges the resulting mixture along the hepatic veins into the ascending cava."

The changes which are constantly occurring in the body during health, in consequence of the growth and renewal of the tissues, must be necessarily admitted to be of a nature calculated to affect the circulation of the blood; and the attraction subsisting between the different organs and certain constituents of the blood, or certain compounds formed and existing in the blood, may therefore be supposed to be alone sufficient to maintain the circulation in animals possessing a less complicated organisation than man. But I must beg to differ from Dr. Draper's interpretation of this force, and to deny that the circulation of the nutrient fluid in man, can be proved to be in accordance with the law which he has laid down. Nor can I agree with the subordinate office which Dr. Draper has assigned to the heart; in supposing that its use is simply, as he states, to intercept, at a central point, "the current going to the respiratory machine and that going to the system, by an apparatus which could hold both in check, and time the movements of the one to the movements of the other." For the duties which the heart appears to perform are far more important than those of a timekeeper, to hasten or delay, as the case may require, the arrival of the blood on the one side, and its departure on the other.

The organic changes which take place in the animal system are effected by a complicated organisation, which strongly contrasts with the simplicity of the structures by which they are effected in plants; and although the agency at work to produce these changes may be fundamentally the same in each case, yet it is unreasonable to suppose that there is the same facility in its application. For complexity of structure in man, as in a steam engine, seems to involve a corresponding complexity in the application of that force by which the machinery is set in motion. Consequently, although the organic changes may be admitted to be the motive power by which the circulation of the blood is effected, yet it cannot be admitted that the blood is attracted simply by the changes going on in the different parts of the system. For the changes are so great and uncertain in some of the organs of the body, as for instance the stomach, that their attraction for blood would frequently become excessive, and overpower the attraction which other organs might, at the time, possess for it; and there would be, in consequence, an accumulation and arrest of blood in the neighbourhood of the digestive organs after the introduction of food. It is apparently to prevent this, that the sympathetic nervous system has been superadded to the organisation of man and other animals, who possess a complicated apparatus for nutrition. It may therefore be inferred, that before the force generated in the organs appropriated to nutrition can affect the circulation of the blood, it has been previously modified in some way, through the medium of the sympathetic nervous system. Although, therefore, Dr. Draper may be correct in referring the circulation of the blood to the force generated by the changes constantly taking place in the animal economy, yet the force itself is more modified, and its application is less direct, in man than it is in plants.

Before concluding this notice of Dr. Draper's observations on the circulation, I would remark, that it must not be supposed that he was the first to distinguish the effect of a peculiar propelling power on the blood in the capillaries. It was clearly recognized by Haller, who almost foresaw its nature. Others, since Haller's time, have adopted some of Haller's views on this point; but all writers on the subject have failed to give a satisfactory explanation of the phenomenon. The nearest approach to a correct interpretation of this obscure subject, seems to be that suggested by Mr. Paget in his "Lectures on Surgical Pathology,"[c] in which he observes: "We might anticipate too, that as the nervous force has its origin in the acts of nutrition by which the nerve substance is formed, so, by reciprocal action, its exercise might affect the nutritive acts. As (for illustration sake) the completed blood affects all the processes by which itself was formed; so, we might suppose, would the nervous force be able to affect all the acts of which itself is the highest product." Mr. Paget proceeds, however, to acknowledge his inability "to say, whether the influence on nutrition is exercised through sensitive fibres of the cranio-spinal system, or through sympathetic fibres; nor do I (he remarks) think the question can be yet determined."

The solution of this difficulty will depend on our ability to unveil the nature of the organic functions; for, without the knowledge necessary to do this, all attempts to explain the circulation of the blood will be fruitless. Before, however, proceeding to touch the veil which at present covers the nature of the organic functions, I most earnestly desire, that no imperfect illustrations of the subject on my part, may lead any one to doubt its connection with the Deity.

---

[c] 1853, vol. i. pp. 40, 41, and 48.

On Organic Function in relation to Cholera.

It may be assumed that as there is in the structure of all organized bodies sufficient evidence to convince us that there must have been unity of design, so their functions should teach us that there is also unity in their law of action: for as the structure of all organized bodies exhibits special adaptation to the functions they have to perform, unity of design necessarily involves unity in the application of that force on which their life depends and by which their work is done. The result of modern researches, both in experimental physiology and in pathology, fully confirm the correctness of this inference, for they not only show that the varied operations of organic life may all be referred to one law, but that this law is expressed in reflex action. It would be out of place in an inquiry of this kind to discuss the arguments which might be urged in favour of this prevailing principle of action, which appears to govern all the functions of organic life, and to regulate their progress, their maintenance, and their decay. It is enough to state that in the nutrition of our bodies, however dissimilar the materials may be, or however much the products may vary, there appears to be but one force employed and but one end attained; the force employed being developed from cell-function, and the end attained being its reflection to become the motive power in the circulation of the nutritive material, or the blood. The introduction, for example, of food into the stomach, or the admission of air into the lungs, excites in each case its appropriate cell-function, and determines the current of the blood; an increased function on the one side being accompanied by an increased flow on the other. But this combined action can only be maintained within certain limits, for although the constantly recurring liabilities to disturbance in the higher organizations is lessened by the introduction of a nervous system, interposed between the cell-tissue and the vessels which convey blood, yet if the increased development of cell-force is urged to excess, the relation between the blood and the tissues is reversed, and the excess of cell-function, instead of simply quickening the circulation, gives rise to a flux.

In the application of the foregoing principles to explain the relative importance and the sequence of the symptoms of cholera, it will be found impossible, for the present at least, to decide whether the characteristic tendency to death in this disease is primarily due to failure of the circulation, or to the flux from the alimentary canal. The meaning of particular symptoms, so far as regards those which have always been admitted to be chiefly distinctive of the disease, has been already very fully investigated in the preceding pages; and a corresponding inquiry into the comparatively less important symptoms of the disease, such as violent discoloration of the skin, local development of heat, conjunctival injection, and others, would tend to illustrate still more fully the explanation which has been offered of the nature of the disease. But with respect to their relative position or sequence, it is impossible, whilst ignorant of the cause of the disease, to settle their individual claims to precedence; and notwithstanding the somewhat dogmatic opinions which have been advanced on this subject, it is still far from being satisfactorily proved that all cases of cholera begin with what has been called a premonitory diarrhœa; for not only may the approach of the disease be occasionally foreshadowed by a very perceptible alteration in the pulse, but there are some cases, and a few of them have come under my own notice, in which a sudden failure of the circulation has been the first symptom of cholera; and even death has sometimes ensued without any discharge from the alimentary canal, although in such cases an examination after death has revealed that the characteristic flux has occurred and been retained. It is not however of much practical importance to determine whether failure of the circulation in cholera precedes or follows the characteristic flux, for it is chiefly the relation which these two symptoms bear to each other, and the evidence they afford of a deficiency on the one side being attended by a corresponding excess on the other, which it is necessary for us to consider with reference to treatment; and, consequently, the purpose for which this work has been written will be attained, if in attempting to explain the nature of the disease by the study of its chief symptoms, I have succeeded in indicating the principles which should guide us in selecting remedies for its relief.

TREATMENT.

The amount of success which has hitherto attended the use of remedies in the late epidemics of cholera has been scarcely sufficient to attract public notice, or to obtain for any of them the approval of the medical profession. This may be partly ascribed to the well known fact that very opposite plans of treatment seem to have almost the same value in cholera, so far as it is allowable to judge of the usefulness of any of them by the results which follow their application; whilst in the most severe forms of the disease, and especially in such cases as occur when the epidemic is at its height, all proposed remedies appear to be equally powerless in arresting their fatal course. Consequently it may be admitted that nothing satisfactory with respect to treatment has at present been decided upon in cholera. This is well shown in the following returns, obtained by the "Treatment Committee of the Medical Council appointed by the General Board of Health," of the treatment pursued in cases of cholera attended with collapse during the epidemic of 1854; since after making allowance for the varying severity of the disease in different localities, especially with reference to elevation, the period at which the treatment commenced, and the sanitary conditions of the dwellings in which it occurred, the results so obtained are, to say the least, very unpromising; although they are negatively important in showing that the use of even such opposite remedies as castor oil and sulphuric acid is followed by almost the same per centage of deaths:—

Calomel and Opium	59.2	per cent.	of	deaths.
Calomel (larger doses)	60.9	„	„	„
Salines	62.9	„	„	„
Chalk and Opium	63.2	„	„	„
Calomel (small doses)	73.9	„	„	„
Castor Oil	77.6	„	„	„
Sulphuric Acid.	78.9	„	„	„

In addition to the above-mentioned remedies there are many others which have been at one time vaunted, and are now almost forgotten; such as sulphur, charcoal, cajeput and turpentine oils, and the inhalation of oxygen and nitrous oxyde gases. In fact almost every known means by which it has been thought possible to produce a specific effect on the system, every drug by which elimination from the stomach and bowels could be produced by emetics and purgatives or the opposite effect produced by astringents, every appliance, by which the system could be either excited or soothed, that an untutored fancy could suggest or a frightened credulity could for a moment trust to, have been employed in this disease with a like unsatisfactory result. The recorded experience of medical men in all parts of the world where cholera has at any time prevailed, proves indeed that however cheering may have been the success of any internal remedy for the disease, subsequent trials have shown that no continued confidence could be placed in it; for such apparent success was at the best only temporary and uncertain. These unfavourable results suggest two important subjects for consideration in connection with the treatment of cholera, namely, that remedies given internally are inert, either from the power of absorption being lost, in consequence of which they are excluded from the system ; or from the arrest of the circulation, in consequence of which they are detained at their entrance into it. For it is well known that in many states of the system, even not attended with any flux from the alimentary canal, medicines taken into the stomach are either not absorbed at all, or are so very imperfectly; as, for example, in cases of over-excitement of the brain, such as occurs in delirium tremens, acute mania, &c.; but as the general circulation of the blood in these cases is not impeded, absorption readily occurs when the same remedy is introduced into the system by hypodermic injection, whilst the power of absorption may be artificially controlled in these and in other states of the system, either by diminishing the amount of blood in the veins, or by arresting its flow—the one effect being produced by bleeding, and the other by ligature.

In the application of these well-known facts to the treatment of cholera during collapse, it is evident that neither the internal administration of remedies nor their hypodermic injection could be of any service so long as absorption in the one case was checked by arrest of the chylo-poietic circulation, or prevented in the other by the absence of subcutaneous blood. Before such remedies could be of any avail in cholera, it would be necessary to re-establish the circulation of the blood ; which, if only temporarily restored, would allow of remedies being introduced into the system, and would obviate the immediate tendency to death. There appear to be three ways in which it is possible to do this—namely, by bleeding, by saline injections into the veins, and by galvanism. All of these have been somewhat extensively tried in cholera, but only the first has been found to be of any permanent benefit ; for although very striking results have been obtained both from galvanism and from saline injections, and the latter remedy has for a time appeared to re-animate, as it were, a corpse, yet the curious imitation of good produced by such means has not only failed to save the patient's life, but has sometimes even appeared to hasten the approach of death. This is precisely the result which theoretically might be expected to occur, for although the nature of the function performed by the blood-corpuscle is unknown, yet it is evident that no artificial means for increasing the fluidity of the blood by saline injections, or for stimulating the motive power of the vascular system by galvanism, could be useful, so long as the tendency of the disease is to produce a relative excess in this most important constituent of the blood. Hence such attempts to restore the circulation ultimately fail, not because the blood cannot be rendered fluid, but in consequence of the blood-corpuscles remaining in undiminished excess, whilst the motive power is exhausted by these one-sided efforts to restore it; for saline injections, as well as galvanism, in so stimulating the blood-vessels, serve only to deepen the consequent exhaustion.

With respect to the effect of bleeding in cholera, which it is proposed to re-introduce to the notice of the medical profession, it will be useful to contrast the opinion formerly and very generally entertained in its favour, with the modern prejudice against it; for whilst the former opinion is based on very strong evidence in support of the practice, derived from the immediate improvement in the patient's condition, and the large per centage of recoveries, the latter rests almost exclusively on the theoretical and apparently fallacious argument that the patient is too weak to bear it. It may be safely asserted that this "scarecrow of imaginary debility" is unsupported by any sound argument in its favour. For the condition of the blood in cholera is relatively that of hyperæmia, and is directly opposed to the constitution of the blood in anæmia, since in the one case there is an excess and in the other a deficiency of red globules. Now as it is a recognised maxim in medicine, with those at least who bleed, that whenever there is a great relative excess of red globules, venæsection is indicated, the arguments which might be urged against the practice in anæmia are favourable to it in cholera. So strongly was this fact impressed upon the earlier observers of the disease, that blood-letting was urgently recommended as a means for "removing the universal depression under which the system laboured." The argument from analogy may be still further urged against this plea of debility as a reason for not bleeding in cholera, for it is in the cold stage of acute intermittents, which many writers have thought to be closely allied to the stage of collapse in cholera, that bleeding has been had recourse to "in many instances with marked and decided benefit;"* whilst even in cases of extreme prostration of the nervous system, in which blood has been already lost, such for example as in gunshot wounds of the abdomen, depletion has been proved by Hennen and other military authorities to be the great secret of their successful treatment. Moreover as the so-called debility in cholera undoubtedly depends upon causes independent of the loss of even a single blood-corpuscle, and as it is especially liable, in those cases in which bleeding has been neglected, to be followed either by hæmorrhage from the bowels,† which is an indication that venæsection was needed; or by febrile reaction, far more common now than formerly, in which the most violent and fatal form of inflammation rapidly passing into gangrene of the alimentary canal, is often the result, and which is well known to be the chief cause of the increased mortality from cholera in modern times, the final argument against bleeding in this disease, to which Dr. Gull has given expression in his report, namely, that "its general inadmissibility is to be inferred from its almost entire disuse in the last epidemic," is based on a fallacy. For apart from the illogical character of the objections here raised, which assumes that a remedy is useless because it is disused, and therefore by parity of reasoning asserts that what continues to be used is in consequence of use—a doctrine which would at once sanction all the vagaries of quackery, and is therefore opposed to rational medicine—it might be justly urged that the term disused here spoken of is an erroneous expression, since very few if any of those who decry the practice have ever had any experience of its effect, or their experience has been limited to the

---

* Statistical Reports on the Health of the Navy for the years 1837 to 1843. Part 2. East India Station, p. 69.

† This occurrence, as a sequel to cholera, has usually in northern Europe been fatal. Dr. A. Vetter ("Lancet," 1834-5, vol. 2, p. 325) in his observations on cholera in

---

Norway in the year 1833, states that "it frequently supervened when Dr. Steven's method (Salines) was employed," and was then "constantly mortal." Among other observers, Dr. Lawrie ("Medical Gazette," vol. 10, pp. 666-670, 1832), physician to the Glasgow Cholera Hospital in 1832, considered it the most prominent of the secondary symptoms, having met with it in upwards of forty examples, and he regarded it as "almost a mortal symptom."

results produced by very small, late, and generally, therefore, useless bleedings in this disease.

The source of nearly all the prejudice against bleeding in cholera may be referred to a habit, very common in our profession, and which has been indulged in perhaps to a greater extent in this than in almost any other disease, of treating diseases by remedies directed against special symptoms. The futile attempts in cholera to restore warmth to the skin, to oxygenate the blood, to check the flux or to alter its character, to artificially distend the contracted bladder (Piorry), to excite the nervous system, and to allay pain, may all be traced to the fallacious, and, as regards cholera, the dangerous opinion that the arrest of one or more of its symptoms is equivalent to the cure of the disease. For, even assuming that it is possible in this way to cope with individual symptoms, the tendency to death from this disease is not in consequence checked; for the admitted increase of mortality in the latter compared with the earlier epidemics of cholera—an increase which is due to the peculiarly fatal character of the secondary fever in modern times, and which, in a large proportion of the metropolitan and provincial cases referred to in the Report of the Board of Health for 1855, occurred without the patient having passed through the stage of collapse—indicates either that the remedies now exployed to check individual symptoms have a tendency to develope this fatal result, or that it is to be ascribed to the almost entire disuse of bleeding alluded to by Dr. Gull.

On referring to the recorded experience of those surgeons who, in former epidemics, have had recourse to bleeding in cholera, it is important to notice that its neglect in modern times is less due to any doubt of its usefulness or to any want of success on the part of those who have more or less extensively practised it, than it is to the disapproval of those who, from prejudice or mistaken theory, have feared to try its effect. It is indeed satisfactory to find that the practical experience of those who have relied on bleeding is not only very conclusive in its favour, but that the success has at all times and in all epidemics been gained in face of an opposition which has pursued the practice with unremitting hostility, and which, in our own day, has succeeded for a time in bringing about its almost " entire disuse."

In attempting therefore to revive a practice unjustly condemned, and to efface the unfavourable impression which a long continued opposition has produced, it will be useful to ascertain the results which have been obtained from this mode of treatment in former epidemics of cholera, before referring either to the results of my own limited experience, to its effects both in the disease generally and on special symptoms, or to the different methods which have been proposed for abstracting blood. For these results will tend to prove that the almost entire disuse of bleeding in modern times, is opposed to a vast amount of trustworthy evidence in favour of the practice; and that, as a rule, where bleeding has been omitted, the mortality has been increased.

The first case in which bleeding was employed in cholera appears to have occurred at Sylhet, in May, 1818, when it is reported that "a Sepoy was attacked, in whom the sickness and retching were so violent, that nothing would even enter the stomach. A large vein was opened, and as the pulse was still firm, twenty ounces of blood were taken away, until deliquium ensued. The best effects were instantly apparent. The vomiting ceased, opium and calomel were given and retained, and after taking one dose of castor oil, the patient on the fourth day returned to his duty."* Subsequently to this case, venæsection was in the first instance employed in every case in which the pulse could be felt at the wrist; and whenever the blood flowed freely, the sickness, spasms, and burning sensation at the scrobiculus cordis were relieved. Throughout the Bengal Presidency the practice was generally approved of, and Jamieson† has recorded that when bleeding was resorted to under favourable circumstances, "it was more successful than any other remedy in cutting short the disease."

In the Madras Presidency, Scot‡ reports that bleeding in cholera "has afforded a signal triumph to the medical art. It requires no common effort of reasoning or reflection to arrive at the conclusion that, when the powers of life appear depressed to the lowest degree, the pulsation of the heart all but extinct, the natural heat of the body gone, and the functions of the system suspended and incapable of being revived by the strongest stimulants, the abstraction of blood might yet prove a remedy against a train of symptoms so desperate. Bleeding was no doubt first employed in cases where there was spasm and where the powers of the system had not much declined; the relief was generally obvious and immediate, and the practice in such cases was thus established. . . . but the employment of blood-letting. . . . as a remedy for collapse in cholera, must have been the result of reasoning and reflection, founded on the general principles of the science; a result highly honourable to the profession, and, as we shall endeavour to show, of the utmost practical importance in the cure of the disease." In the same Presidency, Annesley* bled in cases of cholera with the greatest success, and stated that "bleeding should never be lost sight of"; he called it his "*sheet-anchor*," and referred to cases "wherein blood, drawn even in an advanced stage of the disease, has continued to flow till the balance of the circulation was restored, and the patient recovered."

---

* Bengal Report on Cholera, 1820, pp. 208-9.

† *Ibid.*, p. 246.

‡ Madras Report on Cholera, 1824, preface, p. 58.

In the Bombay Presidency the practice was supported by similarly favourable testimony. Mr. Burrell,† surgeon of H. M. 65th Regiment, states that out of 100 cases of cholera which came under his notice, 88 were bled, of whom only 2 died; and of the remaining 12 cases, who were not bled, 8 died. From the reports sent in by the surgeons of this presidency the following conclusions were drawn :—"At the commencement of the disease in Europeans blood-letting is the sheet-anchor of successful practice; and perhaps also with natives, provided it be had recourse to sufficiently early in the disease; and as long as the vital powers remain, so as to be able to produce a full stream, it ought perhaps never to be neglected, it having been sufficiently proved that the great debility so much complained of is merely apparent."*

---

* Op. cit. pp. 167-8.

† "Bombay Reports on Cholera, 1819." Letter No. 18.

It was in consequence of this concurrent testimony respecting the value of bleeding in cholera from the three Presidencies of India, in which, mainly through the influence of Corbyn, the practice had not only been generally adopted by the medical profession, but had also been officially recommended by the Governor-General, and which was supported moreover by the verbal evidence of distinguished members of the profession formerly employed in different branches of the medical department in India, that the London College of Physicians was induced to sanction the practice in the report which it was commissioned by the Board of Health to draw up on the first expected invasion of the disease in this country. In this report it is stated that the remedy which has been "most uniformly successful, when it could be used, is bleeding, and this even in cases where the pulse was scarcely perceptible at the wrist." The indications to be fulfilled by bleeding, the extent to which it should be practised, and its effects on the disease, were very fully and correctly described; and the support thus given to the practice appears to have been to some extent at least justified by the experience of those who had the moral resolution to adopt it; although for the most part timid counsels were allowed to prevail, and the practice fell into disrepute. Among those who, at this period, have reported favourably on bleeding is Mr. Ainsworth,* who states that in all the cases of cholera which he saw at Sunderland in November and December, 1831, "when a large and copious blood-letting was obtained, the termination was favourable." In the official report of 13 cases of cholera at Sunderland and Newcastle in October and November, 1831,† the only case of recovery was that in which the patient was bled to the extent of sixteen ounces; in two of the other 10 cases, the patients were also bled, but in one of them it was done at too advanced a period to be useful; and in the other case, the bleeding was limited to eight ounces, and as the blood so obtained flowed in a stream of two distinct colours, part being very dark and the rest florid, it is evident that the bleeding should have been practised to a greater extent. Mr. Collyns,[*] of Kenton, near Exeter, in his reply to the General Board of Health, states that "bleeding was found to be most beneficial *in all cases*," where he was called sufficiently early to adopt it. Whilst the report of Staff-surgeon A. C. Colclough[†] to the Army Medical Board, dated Dublin, 27th July, 1832, contains a table of cases which shows very clearly the good effects of large bleedings (22 ounces to 40 ounces) in this disease, contrasted with the fatal results of a very small bleeding in one case and of no bleeding in two other cases.

---

* "Bombay Reports on Cholera, 1819," preface, p. 38.

---

* "Observations on the Pestilential Cholera." London, 1832, p. 148.

† "Lancet," 1831-2, vol. 1, pp. 543-553.

Bleeding was recommended by the Russian Government in the first epidemic of cholera in Russia and Poland, and it was successful in a large number of cases; but the order was subsequently countermanded, it is alleged, because blood, in many cases, would not flow. It is important, however, to notice, in connection with this countermand, that it was at this time that the consecutive fever, which has since proved so fatal, first attracted notice.

In the Austrian empire the practice was, to a large extent, successful. Mr. Fergus,[‡] in his report on cholera at Vienna, in 1831 and 1832, states that "in the first and second forms" of the disease, "bleeding always produced relief:" whilst Sophionopouli[§] strongly rebukes the false cries of weakness from failure of the pulse, and justly reprobates the neglect of bleeding on that ground; remarking that cholera, at the commencement, "yields sometimes, as if by enchantment, to a general bleeding."

---

[*] "Lancet," 1832-3, vol. 1, p. 113.

[†] "Medical Gazette," vol. 10. August 18, 1832, p. 641.

[‡] "Lancet," 1831-2, vol. 2, p. 360.

[§] Op. cit. p. 150, and p. 124.

During the first epidemic of cholera in France, it was thought that general bleedings would be most efficacious in the period of re-action; and although some physicians bled their patients at the outset of the disease, as for example, M. Cazol,* who considered bleeding to be more generally useful in cholera than in any other epidemic disease, yet the practice of early bleedings met with very few advocates, and the consequent mortality from the disease during the stage of re-action, is admitted to have been very large.

In Egypt, the celebrated Clot Bey employed early and large bleedings in cholera with success; and similarly favourable results were obtained " at Cairo, by MM. Cherubini and Raymondi, at Abou-Zabel by M. Duvigneau, at Kanka by M. Chaidufau, at Mansoura by M. Terranova, at Alexandria by M. Rigaud, and by thirty Arab surgeons employed in different places." Clot-Bey reports that "experience had so convinced the people themselves of its efficacy, that the sick, of their own accord, had themselves bled on the appearance of the first symptoms;" and he records the following remarkable fact:—"a lieutenant-colonel of the artillery corps, the surgeon of which had deserted his post, bled with his own hand more than one hundred soldiers at the beginning of the disease, and saved nearly all."*

---

* "Instruction Pratique," 1832, p. 37.

At later periods in the history of the disease, we find the same favourable testimony respecting bleeding. Thus in the reports on Asiatic Cholera in regiments of the Madras army from 1828 to 1844, Dr. Lorimer (p. 48) states that he had recourse to bleeding in 34 of the 51 cases recorded, of which 21 recovered and 13 died; of the remaining 17 cases which were not bled, only 5 recovered and 12 died ; the average quantity of blood drawn in the 21 who recovered was 8 ounces, and in the 13 who died 4: and excluding the quantity drawn from 3 of this number who got over the stage of collapse and died from secondary fever, the average quantity in these 10 is reduced to 3 ounces." Lorimer remarks, in favour of bleeding, that "were I called upon again to treat the same form of the disease, I would not hesitate to have recourse to it in every case, and at any period of this stage of collapse, prior to the appearance of the cold clammy state of the skin; and even when this fatal symptom was present, I have seen it prolong life, and would endeavour by every means to remove a portion of this morbid blood from the oppressed heart, whereby to enable stimulants to act." Dr. George Fife,† late physician to the Sunderland Infirmary, reports that "in no cases has it (bleeding) been more useful than in those where pulsation at the wrist was either lost or reduced to a mere thrill, where the whole symptoms of collapse were well defined, and especially where cramps and vomiting persisted. Under these circumstances I have used the lancet with marked success." Dr. Protheroe Smith* states that in the first stage of the disease "bold *venæsection* seems decidedly the most rational, and from all accounts the most speedily successful, of all curative means which have been tried." Mr. George Oliver,† in relating eleven cases of cholera (in all of which he bled, and the patient recovered) remarks that "my chief dependence is on bleeding, which, even in the state of collapse, when only a small quantity will flow, and that by drops, the relief obtained has been very evident." And in like manner, M. Gendrin,‡ who had very extensive and favourable opportunities for observing two epidemics of the disease in Paris, earnestly advocates free and repeated venæsection, which he terms "l'ancre de Salut" in cholera.

---

* Fabre, "Choléra Morbus." Paris, 1854, p. 174.

† "The Pathology, Treatment, and Prevention of Cholera," p. 13, 1848.

Lastly, as an evidence that this remedy, which was proposed and almost universally relied on during the first epidemic of the disease in India, still has in that country its successful advocates, notwithstanding the outcry which has been raised against it, it appears that quite recently, Mr. Greenhow,* in his medical details of the siege of Lucknow, reports that "bleeding, *even during collapse*, seemed to answer well in several cases. The pulse returned, and though the effect often passed off, the bleeding was sometimes repeated after a few hours, and with apparent success."

---

* "Cholera; an Enquiry, Physiological and Pathological, into its Proximate Cause." Second edition, p. 30, 1849.

† "Lancet," 1849, vol. 2, pp. 270-1.

‡ "Monographie du Choléra," p. 221, 1832; and "Gazette des Hôpitaux," 17 Décembre, 1853.

The first case in which I ventured to bleed in cholera, occurred soon after my arrival at Calcutta, in medical charge of a detachment of H.M. 70th Regiment in May, 1849. The patient, a well grown able seaman, aged 23 years, was attacked with severe and apparently sudden symptoms of cholera, at 3 P.M. The vomiting and purging were profuse and characteristic, and the cramps very severe. The surgeon, who had been first called to the case, had given a large dose of calomel and opium, which had been almost immediately rejected, and a second dose had in like manner been ineffectual. When I saw the patient at 5 P.M., the cramps were apparently the most urgent symptom from the agonizing pain they occasioned; the collapse was, at the same time, well marked. I at once bled him to the extent of 14 ounces, with immediate relief to the cramps, which were completely checked by the bleeding, and did not return. The subsequent treatment adopted was acetate of lead and opium (a practice which I do not advocate at the present time) with turpentine embrocations. The patient died of secondary fever on the fourth day. In this case the sudden and effectual manner in which the cramps were arrested by the bleeding, and the ease with which medicines were afterwards retained, shewed that the bleeding was not only useful but essential; and it is probable that if the bleeding had been larger or had been repeated, the tendency to excessive re-action, which resulted in secondary fever and death, would have been checked or restrained within safe limits. For since the occurrence of this case, which was valuable as a practical illustration of the correctness of the theory that bleeding in cholera is antagonistic to collapse, I have had many opportunities for observing its effect on the disease, and my experience is that in all cases where it can be employed before the collapse has assumed a hopeless character, it is the most important agent we possess, not only for arresting the more urgent symptoms of cholera, but as a necessary introduction to the use of remedies which would be inert without it.

---

* "Indian Annals of Medical Science" for July, 1858.

Bleeding may be considered to have a twofold influence for good over the disease, which is exhibited, first, in its general effect on the circulation, and secondly, in its special effect on individual symptoms. Respecting the first of these effects, it is evident, from the authorities which have been cited in favour of bleeding, that the most marked results following its successful employment depend on restoration of the circulation. This important fact is fully substantiated by all who have had opportunities for observing the effect of bleeding on the disease; and it is chiefly due to the unloading of the venous system, and the comparative diminution of the red particles of the blood; in consequence of which absorption is quickened, if not even in some cases restored, where such power has been previously suspended or lost. This is the primary indication to be fulfilled in the treatment of the disease ; for without the power of absorption, the loss of fluid by the flux from the alimentary canal cannot be supplied by any amount of liquid taken into the stomach, and even medicines in such states of the system are inert; whilst the artificial introduction of saline mixtures into the veins shews that although the physical state of the blood is a great cause of the peculiar cholera symptoms, it is only so secondarily, and that unless the power of absorption is at the same time restored, it is useless to expect that the results obtained by saline injections will be permanent. Hence also it may be urged that whilst the physical change in the condition of the blood is closely connected with cholera collapse, it is probably due quite as much to an excess of the red particles, as it is to a diminution of the watery constituent. Bleeding might therefore be theoretically inferred to be necessary in cholera to produce (1) a diminution of red particles, and (2) an increase of the watery part of the blood. The combined result being that in place of a progressively fatal thickening of the blood, which kills by stoppage of the circulation—either primarily, as in death during collapse, or secondarily, as in death during the consecutive fever, which follows the prolonged arrest of the blood supplied to the alimentary canal—there is substituted a fluid which admits of the organic functions being, for a time at least, restored; for the blood, in becoming more capable of propulsion by a partial abstraction of its red particles, is at the same time rendered more capable of absorption, and by consequent self-dilution more fitted for the maintenance of organic life.

But whilst the general effect of bleeding on the circulation is theoretically, perhaps, the most important of its results, it has sometimes appeared difficult to appreciate fully the advantages so obtained. Consequently the local effects of bleeding on particular symptoms have probably had the chief influence on the adoption of the practice, for these are not only as a rule more readily observed, but they are seemingly also more immediate in their occurrence. No one, for example, who has witnessed the effect of bleeding on the intense cramps of cholera could for a moment question its control over that urgent symptom. On many occasions I have known even a moderate bleeding to check at once the most severe cramps, and that after all other remedies for the purpose had failed. Dr. Twining who was far from recommending indiscriminate blood-letting in cholera, remarks, that "nothing relieves the spasms of the early stage of febrile cholera so effectually as the lancet." In like manner the sensation of burning heat at the epigastrium, the intense thirst, the vomiting, and likewise the flux, are all more or less arrested or controlled by the use of the lancet; and although bleeding alone is not sufficient, it is often a necessary introduction to other forms of treatment.

After once admitting the necessity for bleeding in cholera, it could hardly be supposed that any doubts would be entertained respecting the best mode of abstracting blood. There has, however, been some difference of opinion on this subject, but the evidence of nearly all practical observers is decidedly in favour of venæsection, and this quite agrees with the theory of the disease. Apart from the consideration of such means as cupping and leeching, which, however useful they may be in the sequelæ of cholera, are, as a rule, inapplicable in cases of cholera collapse, either from the absence of subcutaneous blood, or from the effects of such depletion being local rather than general,* it may, I think, be regarded as a curious anomaly in medical practice that a form of depletion so obviously objectionable as arteriotomy in cholera should ever have had any advocates, as the practice is contrary to all the indications afforded by the phenomena of the disease. It appears, however, that even during the first outbreak of cholera in India, arteriotomy was occasionally practised; chiefly, it is stated, in those cases in which venæsection had failed to afford a supply of blood. Dr. Twining has related the particulars of two cases (101 and 102) in which, failing to obtain blood from the veins, he, in each case, opened the radial artery, and succeeded in getting as much blood as he wished; the blood so obtained was purple, and in one of the cases it is described as a dark purple: the collapse, however, continued to increase, and the patients died without any apparent benefit from the operation. As Dr. Twining elsewhere remarks, with reference to the difficulty of

---

* Dr. Müller ("Einige Bemerkungen über die Asiatische Cholera." Hannover, 1848: quoted in "Med. Chir. Review," January, 1849, p. 16) who was sent by the Hanoverian Government to St. Petersburg to investigate the epidemic of cholera in 1848, states that the treatment which claimed the largest share of success was that pursued in the Military Hospital at Krasne-Selo, four miles from St. Petersburg, where 70,000 men were assembled. As soon as the patient was in

---

the algide stage of the disease, twelve cupping glasses were placed along the spine; the body was rubbed with pounded ice and salt, and a glass of cold water, in which rock salt was dissolved, was given every hour. Even this imperfect mode of temporarily withdrawing blood from the abdominal viscera was found to be of some service; and if instead of waiting till "the patient was in the algide stage of the disease," the practice had been adopted earlier; or, better still, if venæsection had been substituted for it at the outset of the disease, a larger percentage of recoveries might have been recorded.

sometimes obtaining blood during collapse, that it might almost always be procured from the larger arteries, his observations concerning the effect of bleeding in cholera, must sometimes be received with doubt; especially in connection with the statement that in those cases which "proved fatal by the rapid accession of coldness and collapse, bleeding may do harm." In like manner Magendie's objection to blood-letting in cholera was unreasonable, both in theory and in practice; for he opened the temporal artery, and imagined that if bleeding should be practised, it was of little consequence from what source the blood was derived.* Whilst in a case in which Dieffenbach† opened the brachial artery, the circulation was found to be completely arrested, although the patient survived for some hours.

The practice of arteriotomy in cholera has been almost constantly unfavourable, and from a careful analysis of the observations of nearly all who have recorded their experience of it, this result may be referred to one of the two following causes; either no blood could be obtained in consequence of the vessel being empty, or the collapse has been fatally increased by the withdrawal of arterial blood. Mr. Hamilton Bell,* whose opinion respecting the nature of the disease accords with that maintained in this treatise, in strongly condemning the practice, states, "we actually find Indian writers so completely mistaken in the rationale of blood-letting, that they narrate their attempts to remove blood by the temporal artery, when the surface is livid, and after the arteries have ceased to beat."

---

* "Leçons sur le Choléra Morbus, faites au Collége de France," p. 205, 1832.

† It may be useful to notice that the practice of this physician affords an interesting illustration of the difficulty some have in regaining the right path after they have once left it; for by a curious inversion of reason, Dieffenbach had recourse to transfusion of blood in no less than three cases of cholera, all of which were fatal: and this practice of transfusing blood, which is singularly inapplicable in a disease characterised by venous congestion, and which cannot therefore be too strongly condemned, has its supporters even in the present day.

If arteriotomy could ever be considered excusable in cholera, it would be in such a case as that described by Mr. Gordon,† in which a vein in the arm and afterwards the jugular vein were opened without much effect; when, on opening the temporal artery, which bled freely, blood began to flow from the veins, and a sufficient quantity of venous blood was readily obtained: followed, it may be inferred, by a good result, for Mr. Gordon concludes with the statement, "that the pulse always rises under the loss of blood, and the heat returns to the extremities as the circulation returns." But even as a possible help towards subsequently obtaining a full supply of venous blood, it is questionable whether arteriotomy is ever allowable, for in those cases in which venæsection primarily fails, it would be a more rational and a more hopeful plan to restore the circulation for a time by galvanism or by saline injections into the veins; and it is probable that the previous ill-success of these two remedies would be counteracted if blood were allowed to flow from a vein after the circulation had, by such artificial means, been restored. There is no record of any case in which this plan has been tried, but it appears to merit notice; for there is abundant medical testimony to prove that the lives of cholera patients, even in an advanced stage of collapse, have not unfrequently been saved when venæsection has been followed by a full supply of blood: and the limits of time within which it is possible to do this may be artificially prolonged, if the circulation can by such means as galvanism, or saline injections into the veins, be temporarily restored.

---

* Op. cit., pp. 21, 22.

† "Bombay Reports on Cholera," pp. 97, 8.

With respect to the effect of drugs, it is questionable whether one drug more than others can, apart from bleeding, be acknowledged to have any influence for good in severe cases of the disease: for where, as in cholera, there is a general tendency to perversion or arrest of the organic functions, medicines which are given to restore the normal action of one part of the system, independent of any regard for the condition of the rest, must, as a rule, aggravate the disease, unless they are inert. So likewise the various attempts which have been made, for example, to restore warmth to the skin by hot air, hot water, and vapour baths, or by stimulating applications, instead of being useful, have but too often had the effect of deepening the general collapse. But in mild cases, such as in those known as choleraic diarrhœa, and in slight attacks of cholera, in which it may not appear necessary to bleed, the remedy on which I have been accustomed to rely has been turpentine, in consequence of its decided influence in checking the characteristic flux. And even in severe cases, in which the inability of the stomach not only to absorb but to retain, has been remedied by bleeding, and the disease has in consequence been reduced to a more manageable form, turpentine in small, and frequently repeated doses, has often been given with advantage. There are many other remedies which appear to be also useful for this purpose, such as cajeput oil, which is closely allied in action to turpentine; the sulphuric and other acids, vegetable astringents, etc. But in favour of turpentine, it must be conceded that there is one important quality which renders it preferable to the sulphuric and other acids in cholera, namely, in allowing of the exhibition of alkalies and the neutral salts, which are needed in consequence of the change effected in the composition of the blood; and it has so well fulfilled the special indications for which it has been given, that there has been no necessity for substituting any other remedy. The subjoined mixture, occasionally combined with small doses of chloric æther, is that which I have been in the habit of giving in nearly all stages of the disease:

℞. Olei Terebinth: ʒss
Liquoris Potassæ ʒj
Muc. Acaciæ ℥ss
Aquæ Carui ℥vss

"Two table spoonfuls to be taken every ten or fifteen minutes for the first three or four doses, and subsequently less often."

Care should be taken to supply the patient, at the same time, freely with iced drinks; followed by beef-tea, broths, milk, and other diluents, as soon as they can be retained; so that the previous loss of nutritious fluid may, by absorption, be replaced.

In conclusion it may be remarked that it would be contrary to the design of this work to discuss very fully the subject of treatment; and the few suggestions which have been offered on the subject may probably fail to inspire confidence in their application to the more intense forms of the disease, for there is no desire on my part to flatter others with hopes which cannot apparently be realised. Notwithstanding, however, the great tendency to death, and our ignorance of the cause of the disease, there is a useful lesson to be learnt from the history of former epidemics of cholera; since it has been proved that sanitary improvements, especially in the badly ventilated, ill drained, and overcrowded dwellings of the poor in low-lying districts, undoubtedly lessen the severity of the disease, and to some extent also check its diffusion. Whatever treatment may be preferred or relied on in the expected epidemic of the disease, to be effectual it should be had recourse to early; and no treatment can be so appropriate as that which is founded on a careful enquiry into the nature of the disease, which is our only sure guide.

SCIENTIFIC WORKS
PRINTED FOR WALTON AND MABERLY.

I.

Quain's Anatomy. Extensively Illustrated, with larger Figures, for the most part New. The Seventh Edition. Edited by WILLIAM SHARPEY, M.D., Professor of Anatomy and Physiology in University College, London; ALLEN THOMSON, M.D., Professor of Anatomy in the University of Glasgow; and JOHN CLELAND, M.D., Professor of Anatomy in Queen's College, Galway. 8vo.

⁂ Part I., Bones, Joints and Muscles, with a Portion of the General Anatomy. 241 Illustrations. 10*s*. 6*d*.

Part II., Arteries, Veins, Absorbents, Nervous System, and a further Portion of the General Anatomy. 10*s*. 6*d*.

II.

Modern Chemistry, Experimental and Theoretic (Introduction to). Twelve Lectures delivered in the Royal College of Chemistry, London, by A. W. HOFMANN, LL.D., F.R.S., V.P.C.S., Professor of Chemistry in the Royal School of Mines. Small 8vo., 4*s*. 6*d*.

III.

On Urinary and Renal Diseases. A Practical Treatise. Illustrated by numerous Cases, and Engravings on Wood. By WILLIAM ROBERTS, M.D., Physician to the Manchester Royal Infirmary. Small 8vo. 12*s*. 6*d*.

IV.

Practical Dietary for Families, Schools, and the Labouring Classes. By EDWARD SMITH, M.D., F.R.S. Author of the Address on the Dietary Question at the British Association, Bath. Third Thousand. Small 8vo. 3*s*. 6*d*.

⁂ This Edition contains Notices of the South American Beef, of Liebig's Soup for Children, and of Liebig's Extract of Meat.

V.

Erichsen's Science and Art of Surgery. Fourth Edition, Revised and Enlarged. 517 Illustrations. 1 Vol. 8vo. £1 10*s*.

"The excellent arrangement adopted throughout this Work, in the consideration of the multiplicity of subjects included in the wide domain of the Science and Art of Surgery, together with the familiar and lucid style in which it is written, afford a reason for the popularity of the Volume with the Profession, and for the demand for its translation into French and German, and for its reprint in America."—*Lancet*.

VI.

Dr. Garrod's Essentials of Materia Medica and Therapeutics. Second Edition, Revised and much Enlarged. Small 8vo. 10*s*. 6*d*.

"Dr. Garrod's Work is not only an explanation, and in some respects a Commentary on the new Pharmacopœia, but it is a complete treatise on Materia Medica. We must not omit to notice a very useful feature in the shape of a posological table, in which may be seen at a glance the appropriate dose of all the articles of the Materia Medica."—*Edinburgh Medical Journal*.

VII.

Dr. Garrod on Gout and Rheumatic Gout. Second Edition, with extensive alterations. Coloured and other Illustrations. Small 8vo. 15*s*.

VIII.

Hand Book of Skin Diseases for Practitioners and Students. By THOMAS HILLIER, M.D. Lond., Physician to the Skin Department of University College Hospital. With Illustrations from the Parasitic Diseases. Small 8vo. 7*s*. 6*d*., cloth.

IX.

Dr. Walshe on Diseases of the Heart and Great Vessels, including the Principles of Physical Diagnosis. Third Edition. Small 8vo. 12*s.* 6*d.*

X.

Dr. Walshe on Diseases of the Lungs, including the Principles of Physical Diagnosis. Third Edition. Small 8vo., 12*s.* 6*d.*

XI.

Dr. Edward Smith on Health and Disease, as Illustrated by the Cyclical Changes in the Human System. Small 8vo., 10*s.* 6*d.*

XII.

Dr. Edward Smith on Consumption, its Early and Remedial Stages. Small 8vo., 10*s.* 6*d.*

XIII.

Kirkes' Hand Book of Physiology. Fifth Edition, thoroughly revised. Illustrations on Steel and Wood. Small 8vo. 12*s.* 6*d.*

XIV.

Ellis's Demonstrations of Anatomy. A Guide to the Dissection of the Human Body. Fifth and Illustrated Edition. Small 8vo. 12*s.* 6*d.*

XV.

Murphy's Principles and Practice of Midwifery. Second and much Improved Edition. Many Illustrations. Small 8vo. 12*s.* 6*d.*

XVI.

Illustrations of Dissections, By G. V. Ellis, Professor of Anatomy in University College, London, and G. H. Ford, Esq. A Part containing two plates, with letterpress, price 3*s.* 6*d.*, every alternate month.

⁂ *Parts* 1 *to* 18, *each* 3*s.* 6*d.*, *are now ready* (*November* 1, 1865).

XVII.

Mr. Quain on Diseases of the Rectum. Second Edition. Small 8vo., 7*s.* 6*d.*

XVIII.

Dr. Harley on Jaundice, with the Application of Chemistry to the Detection and Treatment of Diseases of the Liver and Pancreas. 8vo., 7*s.* 6*d.*

XIX.

Liebig's Handbook of Organic Analysis, by Dr. Hofmann. Small 8vo., 5*s.*

XX.

Gregory's Handbook of Organic Chemistry. Fourth Edition. Small 8vo., 12*s.*

XXI.

Quain and Wilson's Anatomical Plates. 201 Plates. Royal folio. 2 vols., half-bound morocco, £5 5*s.* Plain; £8 8*s.* Coloured.

⁂ *Sold also in Divisions (separately) as under—*

		PLAIN.			COLOURED.		
MUSCLES (51 Plates)	Cloth	£1	5	0	£2	4	0
VESSELS (50 Plates)	,,	1	5	0	2	0	0
NERVES (38 Plates)	,,	1	1	0	1	14	0
VISCERA (32 Plates)	,,	0	17	0	1	10	0
BONES & LIGAMENTS (30 Plates) . .	,,	0	17	0	1	0	0

LONDON: WALTON & MABERLY,
137, GOWER STREET, AND 27, IVY LANE, PATERNOSTER ROW.

BOUND BY
EDMONDS & REMNANTS
LONDON